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ART. I.—*On the Quaternary, or Post-tertiary, of the New Haven Region;* by JAMES D. DANA.*

I. THE GLACIAL ERA AN ERA OF GLACIERS, AND NOT OF ICEBERGS.

IT is still a mooted question in American Geology whether the events of the Glacial era were due to *glaciers* or *icebergs*. Agassiz and Guyot, who were long among the most active of Alpine explorers, found on their arrival in New England only the effects of glaciers. But American geologists are still divided in opinion, and some of the most eminent have pronounced in favor of icebergs.

The region of New Haven is exceedingly well situated for settling the question, not only as regards its immediate vicinity, but for the whole interior of New England. For in the first place, the region is a wide and open area at the southern termination of the Connecticut Valley;† and this valley is the great central valley of New England, including the larger part of its surface outside of Maine, the Green Mountains making its western border, and the White Mountains and the height of land southward, its eastern. Then secondly, the valley has a north-

* The principal facts relating to the New Haven Quaternary, which I propose to bring out in the article here begun, are included in a paper by the author on the Geology of the New Haven region, published recently by the Connecticut Academy, in volume II, of their Memoirs.

† The Connecticut Valley has a general course from north to south, varying little from S. 9° W. It is occupied by the Triassic (or Triassico-Jurassic) Red sandstone formation from New Haven to northern Massachusetts, and this indicates the position which it had in the Mesozoic era. The Connecticut river leaves the valley at Middletown, taking there a southeasterly course among the metamorphic rocks of the eastern half of Connecticut.

and-south course; so that, whether glaciers or icebergs were the agents, it would, in either case, have guided their great movements, and the New Haven region would have necessarily become filled with registers of the successive events. Being situated on an arm of the Sound, and at the same time extending inland for seven or eight miles along three streams that here have their discharge, the phenomena presented comprise results from both the sea and the rivers, and also from their combined action. On these several accounts, the New Haven region is eminently well situated for deciding the glacial question for all central New England.

A special study of the region during the last two years has led the writer to the conclusion that the effects are solely those of glaciers, or rather, of a *Connecticut valley*, or *Central New England, glacier.** To exhibit all the evidence bearing upon the question would require a full discussion of the whole range of facts presented by the New Haven Quaternary. I propose at this time to mention only a few of the more trenchant and decisive points.

The question is—Was the interior of New England in the Glacial era covered by a sea at least 4,000 feet deep for the floating of icebergs; or, was it emerged land as now, (perhaps to a higher level than at present) and submerged only beneath ice, the ice of an immense glacier, 3,000 feet and less in thickness, the ice in Vermont and New Hampshire being 4,000 feet and more above the level of the sea?

1. The sea, if it covered the land in the Glacial era, should have left proof of it along the southern coast of New England.

About New Haven, the height of the stratified Quaternary deposits above the sea (Long Island Sound), or above the river flats, no where exceeds 50 feet. This is the maximum; the particular facts will be stated at another time. Fifty feet is therefore the greatest amount of depression of the land which is indicated, the greatest that can be admitted by those who base their geological conclusions on facts. There are no traces of sea beaches or any thing corresponding thereto at a higher level. On passing the level of 50 feet above the Sound there is an abrupt transition from the stratified to the unstratified drift, except along the courses of streams, and near these, there is the same transition on passing the level of 50 feet (or less) above the existing river flats.

The highest sea-level of the Quaternary era about the New Haven region is thus definitely marked. It is often assumed

* The existence of essentially independent glaciers, (under the continental Glacier) in the Glacial era was recognized by the author in an article in this Journal, vol. xxxv, p. 243, 1863, and in his Manual of Geology, p. 763, published about the same time; and four great glaciers were there particularly mentioned, the Connecticut River, the Penobscot, the Hudson River, and the Mohawk River.

that the *unstratified* drift might have been dropped over the hills by icebergs; but, as Mr. Croll has recently urged, depositions in water cannot be made without stratification. The slightest jar of a vat of water holding sediment suspended will cause that sediment to go down in a laminated state; and so deposits of sands and gravels in the ever moving sea will always manifest their aqueous origin. The stratified and unstratified material about New Haven therefore mark the limit between aqueous and dry-land deposition. Much has been said about "modified drift," or more or less stratified drift deposits, over the hills; but they occur no where, according to my observation, except along water courses or about the sites of old lakes; they sustain the conclusion that the unstratified drift was essentially a dry land deposition.

2. The magnitude, wide distribution, and regularity of direction in the scratches are evidences against Icebergs.

This is no new argument; yet it has lost nothing of its force by long use. Broad furrows, eight to ten inches in depth, made in trap or granite over long distances, uniform in direction, must have required prolonged abrasion for scores of years, and by an abrading agent not liable to change of course through tides or currents, or to changes of form and thereby of center of gravity from waste in the waters over the rocks. Glacial abrasion has been found so generally wherever the soil has been freshly removed that it may be safely inferred, as has been done for other parts of New England, that the abrasion was universal. Passing from the New Haven region westward, over the high plateau of Litchfield, Warren, etc., 1000 to 1500 feet in height above the sea, the scratches have the same uniformity and wide distribution. Grounded icebergs would be sufficient for such results only in case they spread completely over the wide surface and rested firmly upon its every part,—in which case they would be nothing less than partly submerged glaciers.

3. The scratches are, in some cases, in such positions that only a glacier could have produced them.

For example: on Mount Carmel—an east-and-west ridge in the Connecticut valley depression, about 750 feet in height, nine miles north of New Haven—along one of the steep southern valleys, the surface of trap (dolerite) is extensively abraded, and marked with many large and broad north-and-south furrows. An iceberg, moving southward, which could float over the top of Mount Carmel ridge would not touch bottom in this southern valley, for the part eroded is at least 150 feet below the level of the ridge to the north of it. While this erosion is, therefore, beyond the capacity of icebergs, it would be the natural result of a Connecticut valley glacier.

4. Since icebergs are fragments of glaciers broken off by the sea into which they descend, and since their freight of stones is part of the moraines of the old glacier, the boulders of the New Haven region, and of New England generally, should, on the Iceberg hypothesis, be the rocks of the White Mountains (whose highest peak, Mt. Washington (the loftiest in New England) is 6288 feet high), or of some Green Mountain Peak (over a thousand feet lower), if not from some more distant northern source.

But, in fact, the boulders about New Haven have come mainly from the central part of Connecticut and Massachusetts, and largely from the hills or ledges in the Connecticut valley itself; and not from any mountain summit or ridge either side. They are therefore from *the bottom* of the alleged Iceberg sea, and not from any emerged summits.

These boulders are masses of trap 1000 tons in weight and less; and Mt. Tom and Mt. Holyoke, situated adjoining the Connecticut river in Massachusetts, seventy miles north of New Haven, are the most northerly points from which such masses could have come. The tops of these so-called mountains would have been over 2600 feet below the surface if the Iceberg sea were 4000 feet deep. Other boulders are of the Triassic red sandstone; and these also had their origin *in the Connecticut valley*, south of the northern limits of Massachusetts, for none exists farther north. Others, of large size, some of them ten to fourteen feet in length, are of gneiss and came from a gneiss region, either in northern Connecticut, ten to twenty miles west of the Connecticut river, or just north of this in the adjoining part of Massachusetts. Again, six to eight miles west of New Haven, there are numerous large boulders of porphyritic gneiss, which were derived from ledges less than thirty miles distant to the north or northwest.

The facts show, beyond question, that in the Glacial era, the transported blocks came from the comparatively low regions, in the very bottom of the supposed Iceberg sea, not far to the north of New Haven, instead of from distant and elevated heights to the northeast or northwest; and this was true of all the drift material. The observations of others over New England, as well as those I have made over Connecticut, sustain the conclusion that the sand and gravel of the unstratified drift has not come from remote points, but has been shoved southward by some agent that could gather it up over the breadth of the land and bear it onward to drop it after a few miles, or scores of miles of transportation. All this is evidently impossible work for icebergs.

Since, then, icebergs cannot pick up masses tons in weight from the bottom of a sea, or give a general movement southward to the loose material of the surface; neither can produce the abra-

sion observed over the rocks under its various conditions; and inasmuch as all direct evidence of the submergence of the land required for an iceberg sea over New England fails, the conclusion appears inevitable that icebergs had nothing to do with the drift of the New Haven region, in the Connecticut valley; and, therefore, that the Glacial era in central New England was a *Glacier* era.

ART. II.—*On the Corona seen in total Eclipses of the Sun;* by Professor W. A. NORTON.

IN a communication to the Sept. No. of this Journal, I alluded briefly to the auroral theory of the solar Corona, and referred to publications in which I had advocated it. I propose now to give a brief discussion of the theory.

The grounds upon which I have maintained the auroral origin of the Corona in different publications are the following:

1. The Corona cannot be the permanent atmosphere of the sun, shining by reflected light, since its outline is neither circular nor oval, but exceedingly irregular, and it extends out from the sun many times farther in some directions than in others. The utmost that can reasonably be maintained is that for a small portion of its outward extent, for which the gradation of light is nearly uniform, it may possibly be a solar atmosphere.

2. The natural indications of the aspect of the corona are that it is chiefly composed of separate masses of luminous matter, of unequal brightness and length, radiating out from different points of the sun's limb. The general radiated structure of the corona, and the great comparative outward extent of the luminous radiations in certain directions, have attracted the attention of the observers of all modern eclipses. Some streamers have been seen to extend more than 1,000,000 miles from the sun, while others did not extend to one quarter of this distance.

3. Reasoning analogically from the earth to the sun we naturally conceive the body of the sun to be surrounded by a permanent atmosphere. On the same grounds we should infer that the space exterior to this atmosphere is pervaded, either occasionally or permanently, by auroral streamers, similar to those which at times shoot out many hundreds of miles into space, from the upper atmosphere of the earth.

4. If the luminous radiations of the corona are in fact auroral streamers, we must expect that they will not be permanent in their extent and position. Now it is well known

that such is the fact, for the aspect of the corona has been very different in different eclipses (e. g. eclipses of 1842, 1851, 1858, and 1860). It has even been maintained by some observers that the rays of the corona had a flickering luster, and varied in extent and position during the short period of a single eclipse.

5. Admitting, as we must, the actual radiated structure of the corona, its individual streamers, or luminous radiations, may be conceived either to be permanently connected with the sun, or to be composed of luminous matter actually streaming away from the sun, to an indefinite distance, into space. If we adopt the former idea, we virtually admit that a permanent vaporous atmosphere of sensible density extends from the body of the sun to a distance greater than the sun's diameter, a position that cannot with any plausibility be maintained.* In support of the other hypothesis we have the well established fact that some form of luminous matter, belonging to cometary bodies, when it comes under a certain degree of influence from the sun, is projected or in some manner detached from the nuclei of these bodies, and repelled from the sun, and under the operation of the solar repulsion urged away from them to an indefinite distance, forming the luminous trains by which they are attended. (See the author's papers on Donati's Comet published in this Journal, Jan. and May, 1860, and July, 1861; and the discussion of the Dynamical Condition of the Head of a Comet in the No. for Jan., 1859). To suppose that the rays of the corona are actual radiations of luminous matter, is only to suppose that a portion of the photospheric matter of the sun becomes subject to the operation of the same forces, that we perceive the sun to exert upon a portion of the matter of comets. The luminosity of such radiations may be ascribed either to a reflection of the sun's light, or to electric discharges. Upon this question we shall see important evidence was obtained at the total eclipse of Aug. 7, 1869.

6. If we adopt the auroral theory of the corona, and at the same time admit that the auroral streamers are actual emanations of luminous matter, the following consequences may be expected to follow.

(1.) A portion of the auroral matter emitted from the sun should fall upon the earth's atmosphere, and may furnish the substance of terrestrial auroras, for which no terrestrial origin has yet been detected.

(2.) Upon this view of the possible origin of terrestrial auroras, the close correspondence that has been detected between the periods of the sun's spots and of auroras, should sub-

* According to the recent spectroscopic determinations of Lockyer and Frankland, the solar atmosphere must be of exceeding tenuity in the region of the rose-colored protuberances just above the general surface of the chromosphere.

sist if we allow that the spots are merely the natural result of the supposed discharges of the solar matter, prevailing for a time at certain points of the photosphere; or indeed if we grant that they are in any way the result of these discharges visible in the corona.

(3.) In the wave propagation of the impulsive actions on the ether of space, of the electric discharges to which we may ascribe the material emanations from the photosphere, and in the electric and magnetic phenomena attendant upon the reception and accumulation of the solar auroral matter in our atmosphere, we have a plausible general explanation of the periodic and irregular disturbances of the magnetic condition of the earth, and of their known physical relations to the sun's spots. This theory of the origin of the diverse perturbations of terrestrial magnetism I have elaborated, and followed out into a detailed discussion of the variations of the different magnetic elements in former Nos. of this Journal (viz: for March and July, 1855).

We may add that it derives additional support from the general result arrived at by Prof. Chambers, in his discussion of the "Nature of the Sun's Magnetic Action," viz: that "the mode in which forces originating in the sun, influence the magnetic condition of the earth, is not analogous to the action of a magnet upon a mass of soft iron placed at a great distance from it, but that these forces proceed from the sun in a form different from that of magnetic force, and are converted into this latter form of force probably by their action upon the matter of the earth or its atmosphere." If this be admitted then we must conclude that the perturbations of the earth's magnetic condition, as evinced by the variations of the position and directive force of the magnetic needle, must result either from some action direct or indirect on the earth or its atmosphere, of some form of matter emitted from the sun, or from a wave-action propagated from the sun, or from both of these operative causes combined.

(4.) The streamers of the corona should have at different points of the sun's photospheric surface different directions, parallel to the diverse directions of the magnetic force of the sun at this surface. These directions should be variously inclined, in different heliographic latitudes, to the horizontal lines at the points of the surface, and also to the plane of the sun's equator; like the dipping needle on the earth and the streamers of a terrestrial aurora. In low latitudes the angles of inclination to the plane of the equator should be large, and the streamers proceeding from corresponding points in the two hemispheres, should converge and intersect in the plane of the equator. In proportion as these corresponding streamers proceed from points more remote from the sun's equator, they will intersect

under a smaller angle, and their point of intersection will be more distant from the sun's surface; until at the heliographic latitude of 30° to 35°, they will become parallel to the plane of the equator. Those emanating from still higher latitudes will diverge from the plane of the equator and from each other.*

If these facts be attentively considered it will be seen that the result should be the formation of a luminous appearance extending indefinitely outward from the sun into space, and elongated in the plane of his equator; and that to observers on the earth it would have an apparent form more or less triangular.

The same fundamental conception which accounts for the solar corona, and the physical relations known to subsist between the sun's spots and terrestrial auroras, as well as between these spots and the varied magnetic disturbances occurring on the earth, furnishes then an adequate explanation of the extent, form, and position of the *Zodiacal Light*. In fact we see that the zodiacal light is but the indefinite extension of the corona.

We have here tacitly supposed that the solar emanations consist of magnetic matter projected with great velocity into space, in the directions of the prolongations of the auroral columns, and proceeding on indefinitely in these directions; but if, like the cometic matter, they are exposed to a continual repulsion from the sun, the paths described by the receding particles would be hyperbolas convex toward the sun. The point of intersection of any two streamers proceeding from corresponding low latitudes in the two hemispheres, would in consequence be thrown to a greater distance from the sun, but the general result as to the form and position of the luminous appearance produced, (the zodiacal light) would be the same.

In support of this view of the origin of the zodiacal light we may state that Cassini drew from his observations on the sun's spots and the zodiacal light, made during the interval from 1665 to 1688, the conclusion that a physical connection subsisted between these two phenomena, and that the substance of the zodiacal light was in fact some emanation from the sun's spots. Again, according to Arago, it appears from the entire series of observations at Paris and Geneva, that the zodiacal light varies considerably from one year to another, and that the observed variations cannot result entirely from changes in the transparency of the atmosphere. We shall soon see that the form of the corona, as seen in the eclipse of 1869, and previous eclipses, presented certain prominent features that accord with the theoretical conclusion that the zodiacal light is but the indefinite extension of the corona.

* It is here assumed that the magnetic equator of the sun is coincident with his heliographic equator.

It is proper to state here, that in what precedes we have really been contemplating but different sides of one comprehensive theory, which embraces a connected series of solar phenomena, of which the corona is but one term. The outline of this theory is given in the author's Treatise on Astronomy, revised edition (1867). It is that a portion of the matter of the sun's photosphere is in the habitual condition of auroral magnetic columns; that by electric discharges along these columns, their substance becomes dispersed and in part projected into space; and that this process, wherever occurring, may by a continued dissipation of a portion of the photospheric matter at that locality, eventuate in the formation of a visible spot on the disc; that the photospheric matter thus discharged into space, is in that peculiar condition recognized in cometic matter in which it becomes subject to a repulsive action from the sun (or else to a diminished attractive action, as occurred to a certain extent in the case of Donati's comet, and in that of 1861), and in the act of flowing away is visible in solar eclipses as the streamers of the corona, and at more remote distances as the zodiacal light; that these solar emanations furnish the matter of terrestrial auroras, and when descending in copious showers into the earth's atmosphere, and developing electric currents and disturbing the magnetic condition of the earth, are the determining cause of all the phenomena of "magnetic storms." The apparent structure, and variability of the corona, and, as we shall soon see, the most characteristic features of its form, the form, position, and variability of the zodiacal light, the coincidence of the periods of the sun's spots with the periods of terrestrial auroras, and with those of the perturbations of the magnetic needle, all accord with this general theory. I have also endeavored to show in my papers on the variations of the magnetic elements, that these variations are such as should naturally result from the electric currents in the upper atmosphere, (or what may be called the photosphere of the earth), that would ensue from the reception of the supposed impulsive waves, and material emanations proceeding from the sun. (See this Journal, II, vol. xix, March and July, 1855).

If it indeed be true, that from the fundamental conception of material emanations from the sun similar to those which we know to take place from the head of a comet under the influence of the sun, a connected series of phenomena may be theoretically deduced which have their actual counterparts in Nature, it must be conceded that there is a high probability that this conception is founded in truth, and furnishes the true explanation of the varied phenomena observed.

Results of Observations on the Corona made at the Total Eclipse of August 7, 1869.

The observations made on the Corona on the occasion of the eclipse of 1869, have furnished several striking confirmations of the theory that it is an auroral phenomenon.

1. *The observed form and structure of the Corona.*—I will first adduce the results of my own observations. These were made at Des Moines, Iowa, with the naked eye, and a good opera-glass, and were chiefly confined to the Corona. When the totality commenced, and the beautiful corona stood revealed, like a new creation, against the dark background of the sky, almost the first striking feature that caught my attention was the great inequality in the extent of its outstreaming in different directions, and its consequent irregularity of outline. This outstreaming or luminous radiation, was particularly conspicuous from the eastern limb, *nearly in the direction of the plane of the ecliptic or the sun's equator.* It could be distinctly traced in that direction to a distance from the sun equal to his own diameter. For an extent of some 15° on either side of the ecliptic, individual hair-like streamers, seemingly nearly parallel to the ecliptic, extended out a large fraction of this distance. From the opposite limb, and in the opposite direction, the coronal streamers were conspicuous, but of less extent than in the direction of the ecliptic toward the east. From the polar regions other pointed masses of light extended out to considerable distances, but not so far as those just noticed. They seemed to be composed like the others, of rays or hair-like luminous radiations, more or less distinct. The separate luminous lines appeared to Professor Eastman, from the U. S. Naval Observatory, (who observed the corona at the same station, through a small telescope) to converge more or less. This convergence I failed to detect; but I distinctly noticed that the outstreaming mass from near the north pole of the sun had approximately the form of a triangle with curved sides, convex outward, but the triangular outline appeared as if resulting from the intersections of individual radiations, rather than as being the definite boundary of a stationary luminous mass.

The corona had a white silvery luster, and appeared at times suffused with a delicate rosy tinge, but this was probably a subjective effect. No flickering or variation of the luster of the corona, was observable during the totality. Nor was there any noticeable change in its general form, or in the extent of its luminous radiations, though I carefully watched for such changes. Dr. B. A. Gould, who was stationed at Burlington, Iowa, and other observers, thought that both the luster and extent of the radiating masses, or "star points," underwent material varia-

tions. A similar difference of opinion is found in the reports of observations on previous eclipses. If variations in the brightness and extent of the coronal radiations do actually occur, it is favorable rather than opposed to the auroral theory of the corona; but it is probable that the apparent changes are due to inequalities in the interceptive action of the earth's atmosphere on the light of the corona, and especially on the faint light at its outer boundary.

Professor Harkness, of the U. S. Naval Observatory, in his able Report of Observations on the eclipse, states that the four angles of the trapezoidal outline of the corona were in the middle heliographic latitudes—relying upon the report of another observer; but on a direct examination of the question of the location of these angles, or "star points" of the corona, made since the publication of the Report, he has satisfied himself that their actual position was such as I have above reported it from my own observations.*

In the delineations of the corona given by the observers of previous eclipses, two or more conspicuous outward extensions are generally shown, but the positions of these more projecting parts are seldom given with respect to the equator or poles of the sun. The figure of the eclipse accompanying the Report of P. Prof. Capellotti, of observations on the eclipse of April 15, 1865, made at Chili, is an exception. It shows three principal points of outstreaming of the corona; two lying very nearly in the plane of the sun's equator, and nearly diametrically opposite to each other, and a third near one of the poles of the sun. In the eclipses of 1858, 1860 and 1868, four such points were seen, distributed at about a quadrant's distance from each other. In the eclipse of 1842, but two were noticed, which were diametrically opposite to each other. In that of 1851, there appears to have been no marked deviation from a general uniformity of radiation.

Relying then upon the only definite knowledge we have of the location of the more conspicous portions of the corona, viz. that obtained in the eclipses of 1865 and 1869, we may say that the corona is brighter and more extended about in the direction of the plane of the sun's equator than in any other direction. This striking fact lends a powerful support to the auroral theory of the corona; for, as we have already seen, the streamers proceeding from the lower latitudes on the sun, on opposite sides of the equator, should converge and intersect in the plane of the equator, and for a certain distance

* Professor Winlock, in his report of observations on the eclipse, says, "the photograph of the corona taken at Shelbyville shows a flattening at the extremities of the sun's axis, and an elevation about the equatoreal region." The photographic impressions obtained of the eclipse, at the different stations show, however, but a small portion of the outward extent of the corona visible to the naked eye.

on either side of this plane, and in consequence the corona should appear to extend farther in the plane of the equator, than in other directions. The convergence of individual rays or lines of emanation, we have already seen, was actually noticed by Professor Eastman. It of course may happen that inequalities in the amount of outstreaming on opposite sides of the equator, may throw the more prominent and conspicuous parts of the corona to the one side or the other of the plane of the equator. It will be observed that from our present point of view, the extension of the corona in the plane of the sun's equator is a phenomenon kindred to the much greater luminous extension seen in the zodiacal light; the only difference between them being, that in the former the auroral emanations proceed from lower heliographic latitudes, and intersect nearer the sun.

But it may be asked how are we to explain, on the present theory, the "star points" of the corona over the polar regions of the sun. For these, two reasons may be assigned. (1) If we admit a distribution of magnetism on the sun similar to that which prevails on the earth, the auroral streamers should diverge from each other less rapidly in the high than in the low latitudes. (2) Upon opposite sides of a line of no declination traversing the sun's surface, analogous to that which traverses Russia, the natural directions of the streamers prolonged upward would be such as to occasion the convergence and intersections of those proceeding from the opposite sides of this line.

We may say then that the more extended portions of the corona, in the eclipse of 1869, were over those regions of the sun's surface, and those only, where upon the present theory the intersections of streamers might be expected to occur.

In some eclipses distinct luminous curves having the appearance of luminous jets issuing tangentially to the sun's limb, or obliquely inclined to it, and pursuing a course either convex or concave to the limb, have been seen. According to M. Liais these peculiarities were conspicously observable in the eclipse of Sept. 7, 1858. While it is possible that such curves may be the result of the intersections of a mass of straight streamers, it is not improbable that they may be actual luminous jets; for if from any cause any portion of the auroral matter should be projected from the sun in a direction oblique to the surface, it would proceed in a convex hyperbolic curve if repelled by the sun, and in a concave curve if attracted. Now I have shown in a former No. of this Journal (July, 1861), that the portion of cometary matter posited on the convex side of the tail of Donati's Comet was actually repelled by the sun, while that on the concave side had become detached from the head of the

comet, because of a diminished gravitation toward the sun. Upon our fundamental conception that the coronal matter is essentially in the same physical condition as such cometic matter, and subject to the action of the same solar forces, it may well happen that some individual jets will proceed in convex, and others in concave curves, according as the escaping matter is repelled or attracted by the sun.

2. *Observations on the physical constitution of the corona with the Spectroscope and Polariscope.*—The results of the observations made at the late eclipse, with the spectroscope and polariscope, are strongly confirmatory of the truth of the theory of the corona under discussion. Professor Pickering in the Report of his observations with a polariscope, says, "The form of polariscope used was that adopted by Arago in his experiments on sky polarization. It consists of a tube about twenty inches long and two inches in diameter, one end of which is closed by a double image prism of Iceland spar, and the other by a plate of quartz. Looking through the former we see two images of the latter, which when the light is polarized assumes complementary tints. If, now, the corona was polarized in planes passing through the center of the sun (as is generally admitted), when viewed through the polariscope, in one image the upper and lower parts should have appeared blue, and those on the right and left yellow; while in the second image these colors would be reversed, the yellow being above and below, and the blue on the sides. In reality the two images were precisely alike, and both pure white, but one was on a blue and the other on a yellow back-ground. From this we infer that the corona was unpolarized, or, at least, that the polarization was too slight to be perceptible."

We may infer from this that the corona is either self luminous or shines by diffuse reflection; since specular reflection produces polarization.*

The testimony of the spectroscope is still more decisive. Profs. Pickering, Harkness, and Young, agree that the spectrum from the light of the corona was a continuous one, or free from dark lines; but containing one or more bright lines. The absence of dark lines indicates that the corona did not shine by the light of the photosphere, reflected either diffusely or specularly from its substance; since such light, after reflection, should, like the direct solar light, have given a spectrum with the Fraunhofer lines. The presence of bright lines, on the other hand, is a direct indication that the corona was self luminous; and therefore that its light was the result either of com-

* The question whether the light from the corona is in any degree polarized or not cannot be regarded as definitively settled. It is to be hoped that the observations to be made on the eclipse of December will remove all doubt on this point.

bustion or of electric discharges. As it is hardly supposable that an actual combustion could prevail at the distance of tens, and hundreds of thousands of miles from the sun's photosphere, in regions where, if any solar atmosphere exist, the results of recent observations with the spectroscope by Lockyer and Frankland lead us to believe that it can only be the faintest possible trace of it, we must infer that the light of the corona is of electric origin.

In the hands of Prof. Young and Prof. Winlock the spectroscope has obtained direct evidence of a physical correspondence between the solar corona and terrestial auroras. Prof. Young observed in the spectrum of the corona a bright line the position of which he gives as 1474 on Kirchoff's scale, and which proves to be in coincidence with a small line marked as *iron* on Kirchoff's and Angström's maps. He remarks that "it turns out also to coincide very closely if it is not (which is much more probable) absolutely identical with a line recently discovered by Prof. Winlock of Cambridge, in the spectrum of the aurora borealis. He also saw two other fainter lines in the spectrum of the corona which coincided quite closely with other lines reported by Prof. Winlock as visible in the spectrum of the aurora. In view of these results of spectroscopic observation he remarks, as follows: "At present it seems pretty likely that the spectra of the corona and the aurora borealis are identical, with only such differences in the intensity of their lines as we might naturally expect, and that very probably the identity extends to the essential nature of the phenomena themselves."

The detection of the same iron line in the aurora and corona, taken in connection with the well established fact that the vapor of iron is present in the photosphere and chromosphere of the sun, and that the magnetic features of the aurora lead to the natural conclusion that some form of ferruginous matter constitutes the substance of auroras, for which no terrestrial origin can reasonably be assigned, conducts to the inference that the terrestrial auroral matter is derived from the sun, and adds to the weight of accumulative evidence in support of the theory I have advocated that the corona is made up of material emanations from the sun.

NOTE.—Some persons have conjectured that the corona might be produced by the passage of the sun's rays through the earth's atmosphere, but it may readily be shown that this is impossible. When one reflects that the half width of the moon's shadow, in the larger eclipses, is as great as the estimated height of the atmosphere, it will be seen that, to an observer on the central line of the eclipse the line of sight will not fall upon the illuminated portion of the atmosphere exterior to the shadow, unless inclined under a large angle to the line of direction of the centers of the sun and moon. The corona, therefore, if of terrestrial atmospheric origin ought to present, toward the middle of the eclipse, the appearance of a halo *entirely detached from the dark body of the moon, and many degrees distant from it.* It ought also to increase in brightness from its inner border, for a considerable distance outward.

Others have imagined that the corona might be attributable to the passage of the sun's light through a lunar atmosphere; but since some of the streamers, or rays of the corona, have been seen to extend to a distance greater than the sun's diameter, this would require the lunar atmosphere to be of vast extent; whereas no decisive evidence has yet been obtained of the existence of any lunar atmosphere capable of producing a sensible refraction, or reflecting a perceptible amount of the sun's light to an observer on the earth.

Perhaps the more prevalent idea, at the present day, is that the corona, with its rays and tufts of light, is a phenomenon of diffraction produced by the passage of the sun's rays along the denticulated edge of the moon. This theory has an air of plausibility, but it is entirely inadequate to account for the great extent of the coronal rays. The fringes produced by the diffraction of light in its passage near the edge of a body appear to the eye of the observer to extend but a small angular distance from the edge. This would be more strikingly true in the case of a distant body, like the moon.

The only remaining supposition is that the corona is either an envelope of some kind permanently connected with the sun, or is made up of material emanations proceeding immediately from the sun. To the large body of indirect evidence *that the corona is wholly a solar phenomenon* that has been obtained, we may now add that of direct observation, since it appears that "an examination of the photographs of totality," obtained at the eclipse of 1869, shows that as the moon advanced the corona was progressively covered.

ART. III. — *On the Duration of Flashes of Lightning;* by O. N. ROOD. (From a letter to Dr. W. GIBBS.)

AFTER the completion of my first set of experiments on the duration of the discharge of a Leyden jar, I became anxious to make some measurements of the duration of a flash of ordinary lightning, which may be considered as equivalent to the discharge of an immense jar with an enormous striking-distance. The results of Feddersen have shown that the duration of the discharge is increased by an addition to the size of the jar, as well as by augmentation of the striking-distance, and as both these quantities are so large with a flash of lightning, it was reasonable to expect that the duration of its discharge would be prolonged in some corresponding ratio. During the violent thunder-storm of last August, which occurred in the evening, I happened to be at a house commanding an unobstructed view of the horizon, and this circumstance taken in connection with the frequency and proximity of the electrical discharges, induced me, although entirely unprovided with apparatus, to attempt a measurement of their duration. A circular disc, five inches in diameter, was hastily cut from white cardboard, while a steel shawl-pin served as an axis, on which it was made to revolve by constantly striking its edge tangentially with the right hand, the pin being held in the left. The *maximum* velocity attainable in this way was always employed. The general indications at the time were that the rate thus obtained was considerably more uniform than might have been expected, and subsequent quantitative experiments have confirmed this

idea. The first experiments were made by observing black figures traced near the circumference of the disc, which was illuminated solely by the rapidly recurring flashes, and it often happened that the figures, with their details, were seen quite as clearly and sharply as though the disc had been stationary; on the other hand, sometimes the edges seemed blurred, as though the disc had moved through a few degrees during the act of discharge. The result being doubtful, the mode of experimenting was quickly changed; about fifteen narrow radial apertures were made near the circumference of the disc, and the flashes and illuminated clouds were observed through these openings, the disc being made to revolve as before. The distance of the eye from the apparatus was about eight inches, and it was of course adjusted so as to obtain distinct vision of the disc. The result was that sometimes the openings were seen quite unchanged in appearance, but more frequently they were most distinctly elongated into well defined streaks some degrees in length. They were observed often and without difficulty, but as farther confirmation I may add that I requested Prof. Joy, who was ignorant of the actual form of the aperture, to state his opinion of their apparent shape while the disc was in rotation. The reply was that they resembled Prince Rupert's drops—a not unfair description of the phenomena in question. Repeated estimates of their size were then made with paper and pencil. Some time afterward I measured the velocity which I could communicate to this disc in the manner above described, by attaching to it a small hollow axis through which the steel pin passed, the disc being then caused to wind up a thread stretched by a small weight. The rate of rotation thus attainable was found to be about twelve revolutions per second, which is a little more than I had anticipated. The average size of the streaks was 9°, corresponding to a duration of $\frac{1}{4\,0}$ of a second. It hence results that the duration of the flashes of lightning on the occasion referred to was in round numbers about $\frac{1}{3\,0\,0}$ of a second, some of them, however, seeming to be confined to smaller limits.

I know of only a single circumstance which might militate against the correctness of the above conclusion, and it is but fair to give it such weight as it may carry. Becquerel has succeeded with some difficulty in observing a faint phosphorescence when an electric discharge is passed through rarefied air, and it is not absolutely impossible that the effects observed by me were due to a cause of this kind.

This point can hereafter readily be decided by observing with a revolving disc, not the distant clouds, but a sheet of white paper, placed so as to receive the light from the electrical flashes.

Columbia College, Nov. 10th, 1870.

ART. IV.—*On the physical condition of a closed circuit contiguous to a permanent and constant Voltaic current; or, on "the electro-tonic state;"* by ALFRED M. MAYER, Ph.D.

IN 1831, after Faraday had made his brilliant and unequaled discoveries of Voltaic and magnetic induction, he gave, in his first series of experimental researches, a section on a "New electrical State or Condition of Matter;" which he designated as the *electro-tonic* state. This can, in a few words, be explained as a certain state of electrical tension produced in a closed metallic circuit by the proximity of another circuit, through which circulates an electric current. The latter circuit being broken, this tension evinces itself in producing, during its disappearance, an electric wave in the closed circuit, similar in character, but opposite in direction, to the one produced on forming the contiguous electric current.

Faraday, with his habitual philosophic reserve, hesitating to give a formal enunciation of any hypothesis as to the electrical or molecular condition of the wire during this state, says, (Exp. Res. 71) "this peculiar state appears to be a state of tension, and may be considered as *equivalent* to a current of electricity, at least equal to that produced either when the condition is induced or destroyed." But in the celebrated Bakerian lecture which he delivered in Jan., 1832, he reluctantly releases his mind from this opinion, and says, (Exp. Res. 231; 242), "The law under which the induced electric current excited in bodies moving relatively to magnets, is made dependent on the intersection of the magnetic curves by the metal being thus rendered more precise and definite, seem now even to apply to the cause in the first section of the former paper; and by rendering a perfect reason for the effects produced, take away any for supposing that peculiar condition which I ventured to call the electro-tonic state. * * * * Thus the reasons which induce me to suppose a particular state in the wire have disappeared; and though it still seems to me unlikely that a wire at rest in the neighborhood of another carrying a powerful electric current is entirely indifferent to it, yet I am not aware of any distinct *facts* which authorize the conclusion that it is in a particular state."

In Dec., 1834, however, we see with what reluctance he gave up his first opinion, for he thus writes (Exp. Res. 1114) in reference to "the influence by induction of an electric current on itself, and on the inductive action of electric currents generally." "Notwithstanding that the effects appear only at the making and breaking of contact, (the current remaining unaffected, seemingly, in the interval), I cannot resist the impression that

there is some connected and correspondent effect produced by this lateral action of the elements of the electric stream during the time of its continuance. An action of this kind, in fact, is evident in the magnetic relations of the parts of the current. But admitting (as we may do for the moment) the magnetic forces to constitute the power which produces such striking and different results at the commencement and termination of a current, still there appears to be a link in the chain of effects, a wheel in the physical mechanism of the action, as yet unrecognized." Again, in 1838, (Exp. Res. 1661) he recurs to the idea of the electro-tonic state, in speaking of "the transverse effect of a current;" and in his "Relation of the electric and magnetic forces" (Exp. Res. 1729) he states, "It appears to me possible, therefore, and even probable, that magnetic action may be communicated to a distance by the action of the intervening particles, in a manner having a relation to the way in which the inductive forces of static electricity are transferred to a distance; the intervening particles assuming for the time more or less of a peculiar condition, which, (though with a very imperfect idea) I have several times expressed by the term *electro-tonic state.*"

As late as 1851, Faraday yet hopes that future research may verify his idea of the electro-tonic state, for in his paper "on the lines of magnetic force," after having shown the relation of the magneto-electric current to the electro-conducting power of the substance in which it is induced, he writes (Exp. Res. 3172–73), "All the results described are those obtained with *moving metals.* But mere motion would not generate a relation, which had not a foundation in the existence of some previous state; and therefore the *quiescent* metals must be in some relation to the active center of force, and that not necessarily dependent on their paramagnetic or diamagnetic condition, because a metal at zero in that respect, would have an electric current generated in it as well as the others. The relation is not as the attractions or repulsions of the metals, and therefore not magnetic in the common sense of the word; but according to some other functions of the power. * * * * * If such a condition be hereafter verified by experiment, and the idea of an electro-tonic state be revived and established, then, such bodies as water, oil, resin, &c., will probably be included in the same state; for the non-conducting condition, which prevents the formation of a current in them, does not militate against the existence of that condition, which is prior to the effect of motion. A piece of copper, which cannot have the current, because it is not in a circuit, and a piece of lac, which cannot, because it is a non-conductor of electricity, may have peculiar but analogous states, when moving across a field of magnetic power."

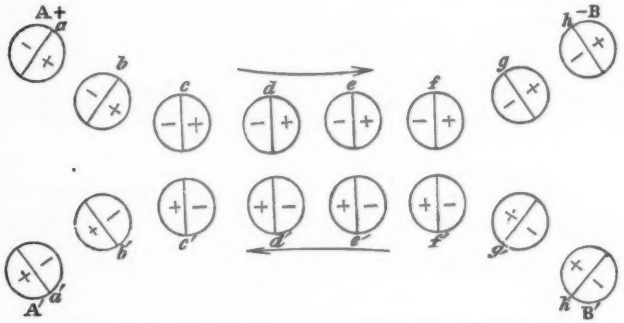
In his paper on the physical character of the lines of magnetic force, occurs this remarkable paragraph (Exp. Res. 3269): "The mutual relation of the magnetic lines of force and the electric axis of power has been known ever since the time of Œrsted and Ampère. This, with such considerations as I have endeavored to advance, enables us to form a guess or judgment, with a certain degree of probability, respecting the nature of the lines of magnetic force. I incline to the opinion that they have a physical existence correspondent to that of their analogue, the electric lines; and having that notion, am further carried on to consider whether they have a probable dynamic condition, analogous to that of the electric axis to which they are so closely, and, perhaps, inevitably related, in which case the idea of magnetic currents would arise; or whether they consist in a state of tension (of the æther?) round the electric axis, and may therefore be considered as static in their nature. Again and again the idea of an *electro-tonic* state has been forced on my mind; such a state would coincide and become identified with that which would then constitute the physical lines of magnetic force."

We see from the above extracts (all that refer to this subject) that Faraday never gave a formal statement of his conception of the physical condition constituting the electro-tonic state; but other electricians, including Weber, Neumann and A. De la Rive, have framed hypotheses, more or less probable, to explain this condition. The one most generally received, and indeed widely copied into existing treatises on physics, is that of A. De la Rive. (Traité d'Electricité, t. i, p. 445). I here give this in his own words; and will then describe certain experiments of precision, which I have made to test the truth of his hypothesis. This is the object of making this communication.

"Without having recourse to conceptions and calculations as profound as Weber and Neumann's, I think that we can consider induction as the result of the ordinary inductive decomposition of the natural electricity of each particle of the induced conductor, by the already separated electricities of each corresponding particle of the inductor. In order that this shall take place, we must admit that the propagation of the current takes place by a series of decompositions and recompositions of the electricities of the successive molecules, in the same manner as takes place with insulating bodies, (referring to Faraday's theory of inductive action).

"Let A B be a conductor traversed by a current in the direction from A to B; the successive particles of which it is composed have their natural electricity decomposed, the − turned towards A where is the positive pole of the apparatus, and the + turned towards B where is the negative pole. The electrici-

ties, as soon as they have been separated, combine from particle to particle, thus: the negative of a with the positive of the pole A, the negative of b with the positive of a, and so on to the positive of h which combines with the negative of the pole B.



This recomposition, which is instantaneous, is immediately followed by a new decomposition, and this by a recomposition, and so on. This succession of decomposition and of recomposition is so rapid, that there is always, as experiment shows, an electric tension in each particle of the conductor, so that we can consider that the state in which it is represented in the figure, which we will call *state of polarization*, is nearly permanent.

"Now let A′B′ be a second conductor similar to the first, as near to it as possible, and insulated with silk or wax. At the moment we pass a current in A B, and where consequently we polarize its particles, we produce in A′B′ an opposite molecular polarization, the + of each particle being opposite the − of each particle of A B and the − before the +. It thereby follows that if, at the moment when A B is traversed by a current, the two extremities of A′B′ are united by a conductor, such as the wire of a galvanometer, the + of the molecule a' combines, through this conductor, with the − of the molecule h', and this produces an instantaneous current directed from A′ to B′ in the conductor, and from B′ to A′ in the wire A′B′ itself, that is to say in a direction contrary to the inducing current. Also, if instead of being united by a conductor, the extremities A′ and B′ communicate with the two plates of a condenser, A′ will give it a charge of positive electricity, and B′ one of negative. As soon as a' has lost its positive electricity and h' its negative, the negative of a' is then disguised by the positive of b', and so on to the negative of g', which is disguised by the positive of h'; these electricities do not neutralize each other, because they are retained by the opposite electricities of the particles of A B; but if at the instant that the current ceases to pass in A B, the

two extremities of A′B′ are united by a conductor, the negative electricity of a' unites with the positive of h', and at the same time the contrary electricities of each of the particles of a', b', c', d', e', f', g' combine, and thence there results a current which progresses in the conductor from B′ to A′ and from A′ B′ in the wire A′B′ itself. Thus A′B′ is traversed, in this case, by a current directed in the same direction as the inducing current. The state of electric tension which exists in the wire A′ B′ whilst the current traverses A B is that which Faraday has called *electro-tonic;* and the cessation of that state produces the second current of induction, while its creation produces the first. * * * * In reality in the above theory which we have given, the production of the two instantaneous induced currents is altogether similar to that which passes in the charging and discharging by cascade of several consecutive Leyden jars, in which the inner coating of each communicates with the outer coating of the preceding one." De la Rive then shows how this hypothesis is applicable to the explanation of the production of induced currents, by the approach and recession of a current in reference to a closed circuit; to the production of induced currents by magneto-electric currents; and to all the facts embodied in Lenz's laws.

It appears to me that the truth of the above hypothesis can be tested in the following manner. A closed metallic circuit (which we will call the secondary wire), is in proximity to a wire carrying a voltaic current, (which we will call the primary wire), which produces in the former powerful induced currents, on making and breaking battery contacts. While in these circumstances, we pass through the secondary wire a direct current, and measure with precision its intensity; then an *exactly equal* current is passed in an inverse direction, and its intensity measured. Now, if as De la Rive supposes, the secondary wire be in a state of polarization like in character but inverse in direction to that produced in the primary by the passage of the current, there will be a diminution in the intensity of the current which traverses the secondary wire in a direction opposed to this polarization, while that traversing it in the direction corresponding to this polarization, will have the same intensity as when it traverses the secondary wire, when the primary current is not in its proximity.

Four conditions have to be fulfilled in the above experiments. First, a strong inductive action must be brought to bear upon the secondary wire; 2dly, this action must be produced by a constant current; 3dly, the secondary wire must be a good conductor, and therefore cannot have in its circuit a liquid battery, wherewith to propagate the current in this wire; 4thly, a means must be devised of producing at will, in the secondary wire, a

current or wave of electricity which shall always have the same intensity. The following apparatus combines these requirements in an eminent degree. Twenty-five spirals (such as are described in my papers in the Sept. and Nov., 1870, Nos. of this Journal) were placed between twenty-five similar spirals, so that each alternate spiral belonged to the same circuit. The terminals of the first series of spirals were connected with a galvanic battery, while those of the second combination were connected with a spiral similar to one of those described in the paper "on the measurement of electrical conductivities," in the Nov., 1870, No. of this Journal. This spiral was placed on a magnet, about 3 ins. distant from its end, so that by suddenly slipping it off a current could be produced in the secondary wire without the interposition of a liquid. By interchanging the connections of the terminals of this spiral, an inverse current could be produced on sliding it off. A reflecting-galvanometer, reading deflections to 15″, was also placed in the above circuit.

If the spiral is always removed from the same length of magnet with the same velocity, it necessarily follows—the circumstances in each case being alike—that successive currents of equal intensity will traverse the secondary wire. This constancy of velocity in all the experiments can be produced in the following manner. The magnet was placed vertically, N. pole downward, and the terminals of the spiral, which were 18 ins. in length, were connected with the ends of a cylinder of copper, transversely divided by an insulating disc of ebonite. Each end of this horizontal cylinder rested on a V, and a close contact was ensured by amalgamation. The spiral was now allowed to fall off the magnet, and as it always made this movement with the same velocity, it followed that currents of equal intensity were produced during each fall. It was however found that currents of such equality of intensity were produced, by *quickly* slipping off the spiral, that the range of their differences did not exceed 20″ in the deflection of the needle of the reflecting-galvanometer. In the experiments which follow, I used the currents produced by quickly removing the spiral from the magnet. This ready method of producing at will an electric current or wave of any required intensity, seems to me will be of value in many electrical researches, for I have found that any current, produced by chemical means, fails in constancy when its intensities are measured by a reflecting-galvanometer, though by the ordinary instrument it may appear so.

In the apparatus thus arranged we have a combination of 25 spirals, containing 367 feet of $\frac{1}{16}$ inch copper wire, in proximity to a combination of 25 similar spirals. A current was passed through the first spirals, of such a strength that the resistance in the battery equalled the outside resistances in the spirals, &c.

After the violent action at the galvanometer produced on making contact at the battery had subsided, and the needle was at rest, its position was determined. The spiral on the magnet was now slipped off, and the deflection of the needle precisely observed. The battery current was now broken; the connections of the terminals of the spiral interchanged, and contact at the battery formed. The spiral was now again removed from the magnet, and the deflection measured. It was found that the deflections of the needle in the first and second experiments gave (for the mean of 6 experiments of each) angles sensibly equal, and that with a galvanometer reading with precision, a deflection to 15″, and whose needle made about three vibrations per minute. These experiments were many times repeated with the current of the battery reversed in the primary spirals as well as in the magneto-current in the secondary spirals, and always with the same results.

Therefore this fact (irrespective of any theoretical consideration) is established:—*that a definite electric current, traversing a metallic circuit in proximity to another traversed by a powerful voltaic current, has the same intensity whether passed in the same direction as the latter or in a direction opposed to it.* There is, however, no doubt, a diminution in the *velocity* of this current similar to that observed in submarine cables; and it will be interesting and important to ascertain whether that velocity is the same in a direct as in an inverse direction. At another time I propose solving this question, and it would be well to reserve until then any hypothesis as to the real condition of a closed circuit contiguous to another carrying a voltaic current. It has always, however, appeared to me that the explanation of dynamic induction given by Prof. W. A. Norton (in this Jour., Jan., 1866,) in his paper on "Molecular Physics" affords a simpler and more consistent explanation of these phenomena than any heretofore framed.

Faraday (Exp. Res. 20 and 33) has also attacked the problem discussed in this paper. He introduced a small voltaic arrangement in the circuit of the secondary wire so as to produce a deflection of 30° or 40° in the galvanometer needles, and then a powerful battery was connected with the primary wire. He found, that after the deflection produced by the induced current had subsided, the needle resumed its former position and such he found was the case whichever way the contacts were made. For the following reasons I do not consider these experiments sufficiently refined to have solved so delicate a problem. First, from many experiments, which I have made, I have shown that no voltaic combination, however constant in the usual acceptation, can be formed so as to hold a galvanometer-needle even approximately in one position for only a minute, when it is ex-

amined by the method of reflection. Secondly, a needle already deflected 30° or 40° has lost its delicacy in showing any minute increase or diminution in the intensity of the current; thirdly, the greatest objection to this method is this very feeble induced current that can be sent through the great resistance of the battery in the circuit of the secondary wire; finally, the manner of reading the galvanometer indications is entirely too gross.

Faraday also (Exp. Res. 3186) approximately showed that when a magnet is quickly introduced into a wire loop or quickly withdrawn from the same that the currents induced in the latter, as measured by an ordinary galvanometer, are of the same intensity; but a deflection on such an instrument cannot be read closer than 15′, while in my experiments I have shown with more refined apparatus that magneto-electric waves can be evolved not differing more than 20″ in the deflections which they produce.

Though ignorance of science excuses one no more than ignorance of the law, yet I may remark that this apparatus was devised and the results obtained before I was aware of Faraday's work in the same direction.

Those who would use this means of obtaining magneto-electric waves of equal intensity must guard against changes of temperature; for both the intensity of the inducing magnet and of the galvanometer needles vary with the temperature, while the resistance of the wire of the spiral and of the galvanometer rises and falls with the same. Indeed if a method could be devised of keeping the wires and the galvanometer needles at a constant temperature while we altered that of the inducing magnet a refined method would present itself for the determination of the variation of the force of magnets with the temperature. Probably the effect of the heat upon the wire circuit could be tabulated and eliminated from the results of such experiments, thus leaving a residual which will express the effect of temperature on the magnet. Or, a differential apparatus might be devised, consisting of two magnets with two wire circuits passing through a differential galvanometer. Having brought the induced currents of the two magnets to equal actions by an interposed resistance, we could then, by altering the temperature of one of the magnets, ascertain its effect with great precision; and it could be expressed in the length of resistance required again to balance the disturbed equality of the magnets.

November 7, 1870.

ART. V.—*Abstract of the Programme for the Observation of Stars to the Ninth Magnitude, undertaken by the German Astronomical Society.**

THE Astronomical Society undertakes the construction of a complete catalogue for the northern heavens of the stars of the first nine magnitudes, upon the basis of the Bonn Durchmusterung. The region to be observed lies between $-2°$ and $+80°$ of declination. The region around the pole is not included, as the labors of Carrington, the Kasan and the Hamburg observatories render a repetition of this work superfluous on the part of the Society. The work will be distributed as follows: Kasan 80° to 75°; Dorpat 75° to 70°; Christiania 70° to 65°; Helsingfors 65° to 55°; Cambridge, U. S., 55° to 50°; Bonn 50° to 40°; Chicago 40° to 35°; Leipzig 35° to 30°; Cambridge, Eng., 30° to 25°; Berlin 25° to 15°; Leipzig 15° to 10°; Mannheim 10° to 4°; Neuchâtel 4° to 1°; Palermo + 1° to $-2°$.

The limits of these zones refer to the equinox of 1855·0, which is that of the Durchmusterung. For purposes of comparison, the limits above given will be exceeded 5′ to 10′, and in very northerly regions even more.

In particular the stars to be observed are as follows: All stars of the D. M. within the prescribed limits having a magnitude of 9·0 or brighter; all stars fainter, which also occur in the Histoire Celeste (marked in the D. M., L), or in the Königsberg zones (marked K), or in the Bonn zones (marked A); a part of these latter, which have been recently determined at Bonn, (marked B). A comparison of the older collections of observations is consequently necessary for completeness.

Every star will be observed twice. Whenever necessary to discover disagreements, a third observation should be made. The observations will be differential in their character, and will depend upon a catalogue of 539 zero stars, to be determined at Pulkova.

To facilitate reduction as well as observation, it is recommended to the observers to divide the zones into subzones. It is deemed inexpedient to observe zones of a length of more than one hour and a half, both on account of the physical fatigue and also the too wide separation of the fixed points of reference. Two zones of one and a half hours each are preferable to a single zone of three hours.

The clock error and pole point will be determined altogether by means of stars from the Pulkova catalogue, mentioned above. At least two zero stars must immediately precede and follow

* Prepared for this Journal by A. N. SKINNER, of the Naval Observatory, Washington.

each zone. Whenever desirable or necessary, however, one or more may be observed during the progress of the zone. In the selection of zero stars the peculiarities of the instrument must govern. In general the mean of their right ascensions should fall nearly on the mean of the zone. Where this cannot be attained, the observer must decide whether he can rely more upon a uniform clock rate (for the determination of right ascensions), than upon an unchanged position of the instrument in reference to the meridian. With very northerly stars the selection should be made more with regard to the declinations of the zero stars, as an error in the clock rate has less influence here. The mean daily clock rate must not be used in the reductions, but the rate resulting from the mutual comparison of the zero stars for the zone.

The individual observer must judge whether a single microscope will give the requisite security in declination. Whenever the stars to be determined are observed on fewer microscopes than the zero stars, a thorough investigation of the reduction between the different systems of microscopes must be made throughout each zone. Where this reduction shows large changes, the use of all four microscopes is advised, and also the introduction of zero stars in the progress of the zone. Changes in the equatorial point may be determined also by horizontal collimators.

Where two share in the work, so that one reads the microscopes, and especially where only one microscope is read, the influence of the warmth of the observer's body must be carefully investigated and allowed for. (Consult Bonn Beob., II, p. viii).

In the observation of double stars the position of the mean is to be obtained. It is therefore advisable to extract from the Positiones Mediæ the double stars occurring in a zone. Should a star designated there as "simplex" appear double, in case of nearly equal brightness, the mean should be observed as well as the brighter one.

An ephemeris of variable stars will be published annually in the Vierteljahrschrift, to furnish information in regard to the most favorable times of observing them on the meridian.

There are many stars of the $9{\cdot}0^{m}$ in the D. M. which are fainter, and it is much oftener so with those marked A. The observer is enjoined to spare no pains in observing these stars, and should the observation not succeed in the course of the zone, note should be made so that it may be attempted under more favorable circumstances. Where variability may be suspected because a star is invisible or excessively faint, it is advisable to immediately inform the committee or those astronomers engaged on variable stars, that these stars may be care-

fully attended to. A notice to the Bonn observatory would render it possible to scrutinize the original papers for any error.

Unless special circumstances prevent, the magnitude is to be carefully estimated at the observation of every star. It is desirable that the participants conform their scale of magnitudes to that of the D. M., unless some other scale is already in use; in that case its relation to the D. M. scale must be determined. (Consult Bonn Beob., III, p. xviii, ff.) To guard against a difference between observations of bright and faint stars, observers are recommended to observe variable stars in different stages of their brightness.

The choice of wire intervals both vertical and horizontal requires mature deliberation. The former must be arranged according to the declination; rather wide for equatorial zones, and rather narrow for northern zones. For eye and ear observing, wire intervals of 10^s to 12^s are recommended. Equidistant wires and in zones near the equator intervals of a whole number of seconds are to be avoided. With the chronographic method intervals of $2^s{\cdot}5$ to 3^s will suffice. The choice of narrower intervals cannot be recommended. The horizontal wires should not be too close, because the fainter stars cannot be seen distinctly through them. A definite precept cannot be given, as the magnifying power employed is closely connected with the question. The observer will do well to ascertain what distance suits him best. Bessel used at first a power of 66 times and afterwards one of 107 times; a third of 119 times, which was perhaps more proper, was not employed on account of its small field of view, a difficulty not to be encountered in the present work, as each star can be directly set upon. The distance of the horizontal wires was $8''$, which is probably rather narrow. The Bonn northern zones were observed with a power of 108 times, those published in vol. vi with one of 150 times. The horizontal wire interval at Bonn is $14''$, which is perhaps too wide; $10''$ to $12''$ may perhaps be more advantageous.

The final catalogue will be reduced to the equinox of $1875{\cdot}0$. It is earnestly recommended to make the reduction of the observations as soon as possible, so as not to be pressed at a later period by the work. Of course a definitive reduction is possible only when the accurate determinations of the zero stars are completed at Pulkova, which will take several years. Meanwhile the provisional places given at present will be so nearly correct that only small corrections will need to be introduced. This reduction will afford an opportunity of judging what security of position is arrived at and where perhaps a third observation is to be added. It will, it is hoped, appear from all the observations that the mean difference between two observations of the same star will not exceed $0^s{\cdot}1$ in R. A. (for north-

erly stars proportionally more), and 1″·2 in declination. This indeed is completely sufficient. It would follow from this that the probable error of a mean of two observations is 0ˢ·04 and 0″·05. But with the above mentioned mean differences among 22 chances the differences 0ˢ·25 and 3″ would occur once, and among 142 chances those of 0ˢ·34 and 4″·1 once. Accordingly it would be advisable perhaps as often as differences occur which exceed 0ˢ·3 and 3″·5 to make a third observation for verification.

The observer will do well to inform himself completely on the subject of the introduction of the D. M., found in Bonn Beob., III. It is there noticed that sometimes two very close faint stars by their united light have given cause to the introduction of a 9ᵐ star which does not exist; such are to be observed if bright enough. If a 9ᵐ star should be found which is not in the D M., and these cases are very rare, it is to be observed and the fact carefully noted, to be published in the final catalogue.

To secure the internal connection and completion of the whole, the participants are urgently requested to transmit, before July of each year, annual reports on the work of the year to the President, or one of the Secretaries, of the Society.

Art. VI.—*On Gahnite from Mine Hill, Franklin Furnace, New Jersey;* by Geo. J. Brush.

The rare species Gahnite has been again found at a new locality in a cross-cut made by the New Jersey Zinc Co. from the valley of the Wallkill river to an opening on the south end of Mine Hill. I collected specimens at this locality in the summer of 1869, and by blowpipe examination at that time determined the mineral to be a zinc spinel.

The mineral differs in its crystalline characters from the specimens of other localities in the frequent occurrence of the cubic plane; in fact the cubic planes are often the largest, so that the crystals are cubes with truncated dodecahedral edges and only small octahedral planes. There are also minute planes of the trapezohedron 2-2, truncating the edges of the dodecahedron; also others of the trigonal-trisoctahedron 3. Besides these there are sometimes two planes between the cubic and the octahedral, which appear, from examination and approximate measurements by Prof. Dana, to belong to the forms 4-4 and 8-8. Their surfaces are rounded, and feeble in luster, and generally they are blended in a single curved plane, consequently the measurements are not entirely satisfactory. The inclinations on a cubic plane, obtained by Prof. Dana, are for 4-4, 160° 30′, for 8-8, 170° 30′.

The crystals vary in diameter from an eighth of an inch to over an inch and a half; generally, however, they are less than half an inch. The color of the crystals is blackish-green; in thin fragments, olive-green. Hardness = 7·5. Specific gravity =4·89—4·91.

Before the blowpipe the mineral is infusible. With the fluxes reacts for iron and manganese; and with soda on charcoal gives a zinc coating.

The analysis in the wet way was made by Mr. Joseph S. Adam of this laboratory. The mineral was decomposed by fusion with bisulphate of potash. The silica was separated in the usual manner, and the iron and alumina thrown down as basic acetates, and this precipitate was examined to insure purity. The iron was determined by titration with permanganate of potash. From the acetic solution the manganese was separated by bromine, and the zinc was thrown down from the filtrate by sulphid of ammonium. The small amount of magnesia was determined as pyrophosphate, care having been first taken to separate the minute traces of it which were found precipitated with the alumina.

Two analyses by J. S. Adam gave:—

	I.	II.	Mean.		
Alumina,	49·86	49·70	49·78	23·20	25·77
Ferric oxyd,	8·83	8·33	8·58	2·57	
Zinc oxyd,	39·39	39·84	39·62	7.82	8·12
Manganous oxyd,	1·20	1·07	1·13	·25	
Magnesia,	0·12	0·14	0·13	·05	
Silica,	0·71	0·42	·57		
	100·11	99·50	99·81		

This gives the relation of the oxygen of $\dot{R}$ and $\ddot{R}$ as 8·12 : 25·77, or 1 : 3·17, which would indicate that a small portion of the iron was present as protoxyd. We have but to assume 1·56 per cent of the $\ddot{F}e$ in the analysis to have existed as $\dot{F}e$ in the mineral to reduce the ratio to exactly 1 : 3.

This variety of gahnite shows a larger percentage of zinc than any heretofore analyzed, and is unique in its cubic habit. It is associated with black mica, apatite, calcite and a brownish variety of chrysolite. A partial analysis of this chrysolite by W. G. Mixter shows it to be a unisilicate of iron, manganese, magnesia and zinc, probably related to, and possible identical with, the zinciferous chrysolite described by Prof. W. T. Rœpper, in this Journal, II, l, 35. A tin-white metallic mineral imbedded in some of the gahnite crystals proved to have the pyrognostic characters of leucopyrite.

Sheffield Laboratory of Yale College, Nov. 1870.

Art. VII.—*Meteors of November 13th–14th, 1870.*

1. *At New Haven.*—On the morning of the 13th of November, 1870, Mr. Charles S. Hastings and Mr. W. B. Roberts, of the Sheffield Scientific School, and Mr. A. A. Murch, of the Academical department of Yale College, watched for meteors upon the tower of Graduates' Hall, in New Haven, from half-past twelve to half-past three A. M. The sky was clear, or nearly so. The following is the result of the count:—

From	to	2 unconf.	1 conf. meteors.
12h 30m	1h 0m	4	2
1 0	1 30	6	1
1 30	2 0	7	1
2 0	2 30	3	1
2 30	3 3	3	0
3 0	3 30	3	0
In 3 hours,		25 unconf.	6 conf. meteors.

The moon was about one sign in advance of the radiant, and evidently diminished very much the number of meteors that could be seen. Probably two-thirds or three-fourths of the whole number were thus made invisible.

On the next morning, that of the 14th, the writer, with Messrs. O. Harger, E. S. Dana, C. B. Dudley, J. McNaughton and R. P. Maynard, watched from shortly after eleven P. M. of the 13th, onward, with the following result:—

Time. h. m.	h. m.	Unconf. met.	Conf. met.	No. of obs.	State of sky.
11 5 to	11 30	6	0	3½	Clear.
11 30 "	12 0	8	1	4	"
12 0 "	12 30	4	5	6	"
12 30 "	12 45	3	5	6	"
12 45 "	1 0	4	5	6	"
1 0 "	1 15	5	3	6	"
1 15 "	1 30	3	9	6	"
1 30 "	1 45	4	8	6	"
1 45 "	2 0	6	1	6	"
2 0 "	2 15	5	7	6	"
2 15 "	2 30	10	8	6	Light clouds.
2 30 "	2 45	5	6	6	Fleecy clouds over all the sky.
2 45 "	3 0	2	2	4	Cloudy.
3 0 "	3 15	3	7	6	Some clouds.
3 15 "	3 30	4	5	6	"
3 30 "	3 45	2	7	6	More cloudy.
Total in 4h 40m,		74 unconf.	79 conf.		

From 3h 45m onward the sky was so nearly overcast that regular counting was abandoned. Here and there open spaces in the clouds enabled us to be assured that up to six o'clock there was not a marked increase in the number of meteors. After half-past five, however, the clouds more nearly covered the sky.

As on the preceding morning, the moonlight was very bright. In the earlier part of the watch there was an aurora, with streamers reaching up half or two-thirds of the way to Polaris.

We called those meteors conformable which moved in paths which if produced backward would cut the curve of the sickle in Leo. But in the practical application of this criterion to tracks distant from the radiant it is probable that any which would strike within 10° of the center of the sickle were called conformable.

Of the apparently conformable meteors, some were undoubtedly accidentally so. The earth's motion being so nearly toward the radiant, a special tendency toward conformability in the sporadic meteors was the result. If we take 15 per cent of the 74 unconformable meteors away from the 79 conformable ones, and add it to the 74, it would be, I think, a tolerable correction for the *accidental* conformability. The numbers would then stand 85 unconformable meteors, and 68 November meteors, or *Leonids* as they have been called.

The only meteor seen by me near the sickle would correspond to a radiant one-third or one-fourth of the way from *gamma* to *epsilon* Leonis. It was moving at a large angle with the line joining those stars.

Many of the conformable meteors had the peculiar light and train which belong to the Leonids.

On the morning of the 15th the sky was overcast at New Haven. Between eleven and twelve o'clock P. M. of the 14th, however, there were a few breaks in the clouds. I think there could not have been any great number of meteors at that time, as I did not see a single one through the openings.

2. *At Brunswick, Me.*—The following is from a letter of Prof. Rockwood of Bowdoin College.

"I was abroad from $0^h\ 45^m$ till nearly 5^h A. M., Nov. 14th, and assisted by two members of the senior class of Bowdoin College, kept a watch and count as given below. The time is local time, as nearly as could be ascertained.

Time.	Conformable.	Unconformable.	Total.
$1^h\ 20^m$ to 2^h	4	4	8
$2^h\ 15^m$ " $2^h\ 45^m$	20	4	24
$2^h\ 58^m$ " $3^h\ 30^m$	25	4	29
$3^h\ 40^m$ " $4^h\ 10^m$	17	4	21
	66	16	82

"We saw 10 or 12 more, mostly conformable, at times when not engaged in the formal count. The sky was mostly clear, but sometimes partly obscured by a few light passing clouds.

"The number of meteors was not large but sufficient to prove the return of the November shower. Especially noticeable

facts were—(1) the almost perfect conformity to the radiant of brighter meteors; (2) the uniformity in the number of the unconformable meteors.

"On the morning of Nov. 15th, 1870, it was cloudy and raining."

Upon the chart sent by Prof. Rockwood are six tracks of meteors observed by himself. The following are their apparent places of beginning and ending.

Time. Nov. 14th.	Beginning. R. A.	Beginning. Dec.	Ending. R. A.	Ending. Dec.
$2^h\ 49^m\ 30^s$	142°	$+40\frac{1}{2}°$	135°	$+51°$
3 25 10	134	$+13\frac{1}{2}$	$126\frac{3}{4}$	+ 8
3 40 30	139	$+30\frac{1}{2}$	$133\frac{1}{2}$	$+35\frac{1}{2}$
3 42 30	145	$+20\frac{1}{4}$	$141\frac{3}{4}$	$+18\frac{1}{4}$
4 4 20	$141\frac{1}{2}$	0	$136\frac{3}{4}$	$-10\frac{1}{2}$
4 9 30	$150\frac{1}{8}$	$-2\frac{1}{4}$	$150\frac{1}{2}$	$-14\frac{1}{2}$

The fourth of these paths was close to the radiant and would, if carried back, cut the line joining γ and ε Leonis *three-sevenths* of the way from *gamma*.

3. *At Burlington, New Jersey*, November 14, 1870.—Mr. B. V. Marsh observed meteors as follows: from $0^h\ 30^m$ A. M. to $0^h\ 45^m$ A. M., looking south from a window, 3 conformable. In the open air, attention directed principally toward the south, sky *perfectly* clear:

A. M.		A. M.	Conformable.	Non-conform.	Total.
From $1^h\ 30^m$	to	$2^h\ 0^m$	4	1	5
2 0	"	2 30	3	0	3
2 30	"	3 0	4	1	5
			11	2	13

Towards 3 o'clock clouds began to interfere materially, and at daylight the whole sky was covered. About half the number were equal to stars of 1st magnitude and several had trains, but there was no one that was worthy of special remark.

Mr. John G. Gummere from $1\frac{1}{2}$ to 3 o'clock, saw 9 conformable and 2 non-conformable; total 11.

Prof. Kirkwood reports a cloudy sky at Burlington, Ind.

Probably Prof. Rockwood and Mr. Marsh had a less sharply marked criterion of conformability than that employed by us in New Haven. As the result of all our numbers, we may say that the number of Leonids visible on the morning of the 14th of November, was probably about equal to, perhaps exceeding, that of the sporadic meteors. In view of the disturbing effect of moonlight this comparison is more valuable than any comparison with the absolute numbers counted in clear nights.

H. A. N.

ART. VIII.—*On some phenomena of Binocular Vision;* by JOSEPH LECONTE, Prof. Geol. and Nat. Hist., Univ. of California.*

IV. *The mode of representing the position of double images.*

IT is well known that if two objects, as a finger of each hand, be placed one beyond the other in the median line of sight, when the eyes are fixed upon the nearer object the farther object is seen double, the images being *homonymous*, i. e. on the same side as the eyes to which they belong: and when the eyes are fixed upon the farther object the nearer object is seen doubled, the images being in this case *heteronymous*, i. e. opposite the eyes to which they belong. These familiar facts are usually represented graphically as follows: Let R and L, figs. 1 and 2, represent the right and left eye, and A and B the two objects. Now when the eyes are directed upon A, fig. 1, then the light from B will impress the *temporal* halves of both retinæ and B will be seen by the right eye at *b* and by the left eye at *b'* (heteronymous); but if the eyes be directed upon B fig. 2, A will impress the nasal sides of the two retinæ, and be seen as homonymous images at *a* and *a'*. (In all cases in this paper objects seen single are represented by capitals, right eye images by plain italics, and left eye images by dashed italics). It will be observed that in both cases the doubled images are referred to a plane passing through the point of sight at right angles to the visual plane. For convenience I will call this the *plane of sight.* Now every one who has ever tried the experiment knows that the double images are not thus referred in natural vision, but on the contrary are seen at their real distance, though not in their real position. The figures therefore though they truly represent the parallactic position of the double images do not represent truly their apparent distance. If on the other hand we attempt in our figures to refer the images to their proper distances, observing the law of direction, then they unite and form one; which is equally incorrect. It is evident therefore that these figures cannot represent truly the visual results.

The falseness of this mode of representation becomes much more conspicuous, if instead of two points or small objects, we substitute a line or rod. In this case the absurdity of projecting the images on the plane of sight is so evident that it is never attempted. The universal mode of representing the visual result of a rod placed in the median line of sight is shown in the accompanying figures. Fig. 3 represents the actual position of the rod AB in the median line of sight, fig. 4, the visual result when the eyes are directed upon A, and fig. 5 the visual result

* For the preceding articles on this subject, see II. xlviii, 68, 153.

when the eyes are directed upon B. Now it will be observed that in both these figures the image of each eye is coincident with the visual line of the opposite eye, and therefore makes an angle with its own visual line equal to the visual angle. But this is not true. Fig. 3 shows that it ought to make but half that angle. If these figures therefore represent truly the position of the images, as indeed they do, then they do not represent truly the visual or apparent positions of the visual lines. The truth is, in natural vision the visual lines are shifted as well as the images of all objects not situated at the point of sight, and to the same degree, so that their positions relative to the visual line are perfectly maintained in the visual result.

Figures constructed on the usual plan give correctly the position and distance of objects seen single, but fail to represent truly the place of double images. They are well adapted to express binocular combinations of similar objects or similar figures on the plane of sight, as in my previous experiments; and to some extent also in the stereoscope, but are unadapted to express the results of binocular vision of natural objects.

I propose therefore a new, and I am convinced, far truer mode of representing the results of binocular vision, applicable to all cases. I am satisfied that if this mode had always been used much confusion would have been avoided. Some preliminary explanation will be required to make the method clear.

If a *single* object, as a finger, be placed before the eyes in the median line of sight and the eyes be directed to a distant point, the object will be seen double, the heteronymous images being separated by a space exactly equal to the interocular space. Now, the *nose* is no exception to this law; the nose is always seen double and bounding the *common field* of view on either side. Again if *two similar* objects or figures be placed before the eyes in the plane of sight and separated by a space equal to the interocular distance, and the eyes be directed as before to a distant point, both objects will be doubled, but two of the doubled images, viz: the right eye image of the right object and the left eye image of the left object, will combine to form a *single binocular image* in the middle, while the right eye image of the left object will be seen to the left and the left eye image of the right object will be seen to the right. Thus there will be three images seen; the middle one binocular, the right one belonging to the left eye alone and the left one belonging to the right eye alone. Now, *the eyes themselves are no exception to this law.* In binocular vision the eyes themselves seem to double; two of them combining to form a *binocular eye in the middle* which looks out between the two noses, while the other two are on either side beyond the noses. Each eye seems *to itself* to occupy the central position while it sees (or would see if the

nose was not in the way) its fellow on the other side of the nose. In other words, in binocular vision when the eyes are fixed upon a distant object the whole field of view, including the parts of the face, is shifted by the right eye one half the interocular space to the left, and by the left eye the same distance to the right, without altering the relative position of parts. By this shifting it is evident that the two eyes with their visual lines are brought in perfect coincidence, so that *identical points* in the two eyes are perfectly united.

The outline of the field of view varies somewhat with the prominence of the nose, brows and cheek bones, but its general form is much the same in all persons. I give in the accompanying figure (fig. 6) a rude outline of the field of view in my own case, $n\,n\;r\,r$ being the field of the right eye, and $n'n'\,l\,l$ the field of the left eye, and the irregular space $nn\;n'n'$ being the common field of binocular vision bounded by the outline of the nose nn as seen by the right eye, and $n'n'$ as seen by the left eye. The circle E represents the position of the combined eyes in the center of the common field, l and r' the position of the two eyes as seen each by the other or rather as they *seem* each to the other. A vertical projection is shown in fig. 7. E being the combined eyes, n and n' the nose as seen by the right and left eyes respectively on either side of the common field, and $l\,r'$ each eye as seen by the other. It will be observed that I have represented the eye on the extreme *right* by r' instead of r, and on the extreme *left* by l instead of l'. The reason is that these, like the two noses, are only heteronymous *images* seen by the two eyes respectively from their central position. As an *organ* of vision the two eyes occupy the central position only, but as an *object* each from this central position sees its fellow on the other side of the nose right and left. This vertical projection I shall use in all my diagrams representing double images. By its use, however, we may represent equally well the position of objects or images seen *single* at the point of sight. By this method the visual results of even the most complicated figures, not only may be represented truly and with ease, but may be *worked out* a priori with the utmost certainty. The great importance of this a priori help will be appreciated by all who have made experiments in binocular vision, and who therefore know how easy it is to overlook, or rather how difficult it is often to perceive, many of our visual impressions.

7.

As we have already stated, while we gaze at a distant horizon any object in the median line *whatever be its distance* is seen double, the space between the images being exactly equal to the interocular space. Evidently then the median line of sight

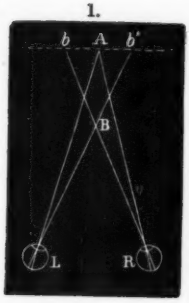
itself is doubled and becomes two lines which, resting upon the nose on each side of the common field of view, run out parallel to each other. *Between these two lines* the combined eyes look out along the *combined visual lines*, at the distant object. If the position of the median line of sight be occupied by a *real* line or rod we shall *see* two parallel lines or rods; if it be occupied by a *plane* as in the stereoscope, we shall see two parallel planes bounding the view on either side, between which we look.

Suppose now we take a slender rod AB *n* (fig. 8) like a straight wire, and placing one end on the root of the nose and the finger on the farther end, so that it occupies the position of the median line of sight, we look at a distant horizon. As already stated we shall see two parallel rods between which we look at the distant point, (fig. 9). If now we look at a nearer point the rods will converge, carrying all objects in the fields of view with them, and meet at the point of sight. If for example we look at the finger A, the rods will form a triangle of which the finger is the point and the distance between the two noses the base (fig. 10); if we look at a still nearer point B, the rods, still turning upon the noses as a fixed point, will cross each other at the point of sight, (fig. 11). At the same time the two eyes with their visual lines combine to form a median eye and a *true median line of sight* (the dotted line in the figures), passing through the point of sight and beyond to infinite distance, and this line might well be called by this name if the name had not been already appropriated for another line.

It will be observed that in the above paragraph I have used very much the same language as other writers on this subject, and used a similar mode of representation; only that I have substituted the eyes in the place of the nose and put noses in the position of the eyes. I have made median lines cross each other at the point of sight instead of visual lines, and visual lines combine in the middle as a true visual median line. In other words, I have used the true language of binocular *vision*.—I have expressed what we *see* rather than what we *know*, the language of simple appearance rather than that mixture of appearance and reality which form the usual language of writers on this subject.

In all the experiments which follow it will be necessary to get the interocular distance with accuracy. This may be very conveniently done by taking an ordinary pair of dividers, holding it up at arms length against the sky or a cloud, and while gazing steadily at the sky or cloud, opening the points until the double images of the points shall two of them coincide perfectly, thus: The distance between the points *b c* (equal to *a a'*) is the interocular distance.

1.



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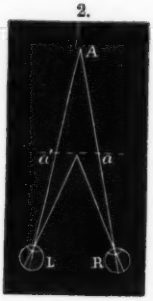

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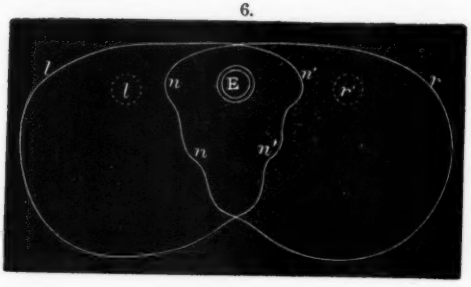

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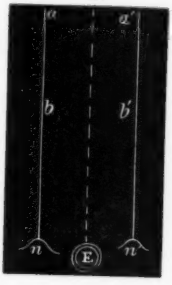

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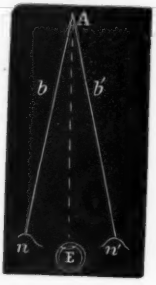

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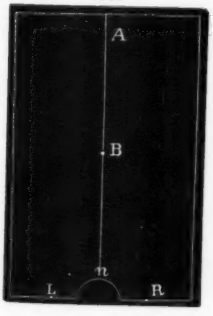

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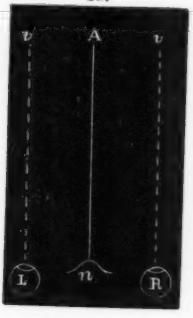

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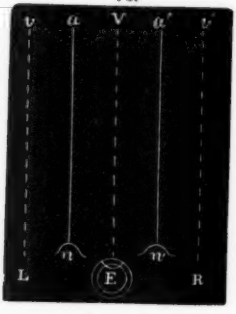

I will now give a few very simple experiments and the visual results, represented in the manner already indicated. Let a smooth white plane about twenty inches long and of any convenient width, be prepared by pasting white paper on a smooth flat board, (fig. 12). Mark two points R and S with a space between them exactly equal to the interocular space. In the middle between the two points notch the end of the board so as to fit over the bridge of the nose. From the notch *n* draw the line *n*A in the position of the median line of sight when the notch is fitted on the nose, and on this line place pins at A and B. If now, the plane be fitted on the nose and held in a horizontal position a little *below* the visual plane, so that both the pins and the line can be distinctly seen, and the eyes be directed upon a distant object; then, as already stated the median line will be seen as two parallel lines separated by a space equal to the interocular space. If in addition to the median line the position of the visual lines be represented by dotted lines, then fig. 13 will represent the *actual* relation of parts, and fig. 14 the *visual* result. It will be observed by comparing the visual result with the drawing, that the parts of the face, the whole plane and all the objects and lines on the plane are shifted by the right eye one half the interocular distance to the left, and by the left eye the same distance to the right; the two visual lines of the drawing are combined in the result to form a true median binocular visual line EV, while the median line of the drawing is doubled, forming heteronymous images, which in the result occupy the position of the visual lines of the drawing. Besides these three lines, there are seen two other dotted lines *lv* and *r'v'*. These are not visual lines proper, i. e. lines along which vision takes place, but they are the visible representatives of the visual lines of each eye seen *as objects* by the other eye.

Next, having removed the dotted lines in the previous experiment, let the eyes be directed upon the pin A, and dotted lines be again drawn in the direction of the visual lines as in fig. 15. The visual result is shown in fig. 16. By comparing this visual result with the actual drawing it will be seen that the whole triangle L A R, is turned upon A as a fixed point a half interocular distance to the left by the right eye, and to the right by the left eye.

Next, having again erased the dotted lines of the previous experiment, and substituted dotted lines crossing at B as in fig 17; let the plane be again adjusted to the nose and the eyes directed upon the pin B. The visual result is shown in fig. 18. In this visual result the whole drawing (fig. 17) is apparently rotated about the point of sight B in opposite directions by the two eyes, through a distance represented by the angle *n*BR or *n*BL =half the visual angle. As objects on the nearer and the farther side of the point B must move in opposite directions,

the double images beyond B must be homonymous. In this experiment (as also in a less degree in the last), since the eyes are converged, the distance between the points L and R *on the plane*, must be a little less than the interocular distance.

In both the above cases the visual result is the drawing rotated about the point of sight in opposite directions through one-half the visual angle; and for convenience I have spoken of the eyes as accomplishing this rotation. But this is not strictly true. The fields of view of both eyes are *habitually* and *normally* shifted one-half interocular space right and left heteronymously, when the eye is in a *passive* state. Under these conditions the visual results of figs. 15 and 17 would be figs. 19 and 20. But in converging the eyes upon A or upon B, the median lines *n b a*, *n′b′a′* turning upon *n* and *n′* as fixed points approach each other, meet and cross at the point of sight so as to form the result already given in figs. 16 and 18.

Lastly—if without using the plane, three objects A, B and C, be placed in the median line of sight, and the eyes be fixed upon the middle one B (fig. 21), then by the *usual* mode of representation the position of the double images of A and C will be *a′a*, *c c′*; the lines here representing of course not visible lines but only lines of direction. But if using the plane we draw a *visible* line in the direction of the median line, and upon it place three pins at A, B and C, (fig. 22), and connect these points with R and L, also by visible lines, and then direct the eyes upon B, the falseness of the former mode of representation becomes at once evident. The visual result of this experiment is shown in fig. 23. Every line of this somewhat complex figure can be distinctly seen. By comparing the two modes of representation (figs. 21 and 23), it will be seen that the parallactic position of the double images, or their position when referred to the plane of B, is the same (the dots represent these positions in fig. 23), but only the second mode (fig. 23) represents truly their apparent distance.

Thus there are two apparent movements of the visual fields accomplished by the eye (or the mind) in binocular vision: 1st, in a *passive state*, a *shifting* of each field one-half interocular space to the opposite side, so as to bring the two visual lines together to form a *true median* visual line; this movement is involuntary and habitual: 2d, in *ocular convergence* a *rotation* of each field in a direction opposite to the motion of the eye, on a line passing through the eye normal to the visual plane; this takes place in every *voluntary* act of sight. In all other movements of the eyes, as when we look from side to side or upward or downward, *objects* seem *stationary*, and we are conscious of the movement of the visual lines over them; but when the optic axes move toward each other as in convergence,

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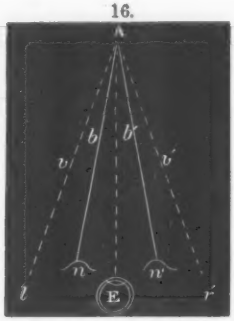

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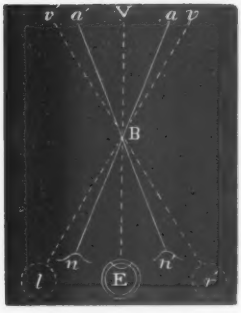

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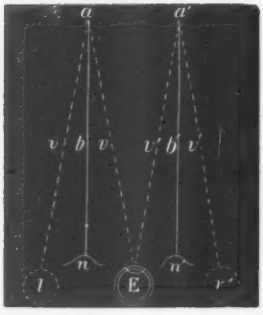

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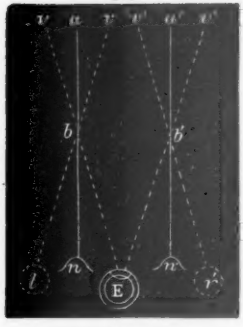

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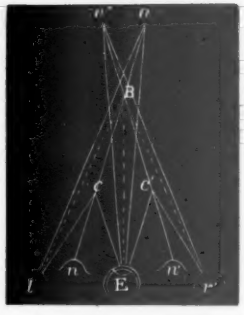

24.

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all objects or images in the fields of view seem to move while the visual lines seem stationary. As the visual line of the right eye moves to the left all the images in its field seem to move to the right, and as the visual line of the left eye moves to the right all its images move to the left. Thus as the visual lines of the two eyes pass successively over objects in the external world right and left of the median line, the images of these objects move forward, and successively take position *directly in front*; so that under all circumstances the two visual lines combine to form a *median* visual line passing through the point of sight and onward to infinity. In case the images thus brought together are *similar*, as in my previous experiments, they combine, and are seen single at the point of sight; but otherwise they maintain their relative distances. This is especially true when we look at continuous lines on a plane parallel to the visual plane, as in the experiments detailed above. The fields rotate toward each other and images come together on the median visual line, but maintain their relative distances from the observer.

I might multiply experiments of this kind without number, but I wish now to show into what singular confusion some of the most careful observers have fallen by adopting the usual mode of representation. I will give first an experiment of Claparède which, according to him, is only a confirmation of what had been previously observed in the last century by Robert Smith, again in 1818 by Vieth, and again in 1838 by Wheatstone. It is therefore well fortified by great names. I translate:*—"Take a compass widely opened (fig. 24), apply its head to the root of the nose, holding the branches in the plane of vision. The screen S is so disposed that the left eye L sees only the branch *a*, and the right eye R only the branch *b*. If now we press the branches until the points are brought on the optic axes, (the point of sight being at A) as indicated in the figure (fig. 24); *we will perceive a closed compass* between the images *s s s's'* of the screen (fig. 25), and this compass stretches itself even to the point of view A." Now I have repeatedly tried this experiment and I find the visual result entirely different. This result is shown by my mode of representation in fig. 26. It is *two half compasses united at the point and separated at the head, by the whole common field of view.* It cannot be otherwise, since we must see two noses bounding the common field of view on each side, and the head of the compass resting on the nose must be similarly doubled and the images similarly separated. As to *length*, the compass may be imagined of *any* extent short of the point of sight A. If the compass is placed *exactly* in the plane of vision, the experiment succeeds equally

* Bib. Univ. Archives des Scien., II, Tome 3, p. 254.

well without the screen, for in that case only the right branch can be seen by the right eye, (the left branch being covered by it), and the left branch by the left eye. If the compass be placed a little below the plane of vision, so that both branches may be seen by each eye, or better if it be laid on the plane before described as in fig. 27, then the visual result will be represented by fig. 28.

The error in the interpretation of this very simple experiment, is due to the difficulty which most persons, even good observers, experience in separating what they *know* from what they *see*. In this experiment we *know* that the nose is in the middle between the eyes; we *know* also that the head of the compass is on the nose; and we *see* that the two points of the compass are together, therefore we *imagine that we see* a closed compass stretching from the nose to this point. But this is not only different from what we really see when our visual impressions are properly analysed, but is contrary to the first principles of binocular vision. The head of the compass impresses the extreme temporal limits of the retinæ of the two eyes, and must therefore not only be seen double, but *the double images are separated by the whole common field of view*. It seems almost incredible that images so widely separated should be imagined single.

Again: in Guy's Hospital reports is found an elaborate series of papers on the "Physiology of Vision," by Joseph Townes, extending through 8 vols., viz: 3d ser., viii and xv inclusive. Mr. Townes' experiments are performed with great care, many of them with the apparatus, and in the manner, already described. He has also *in most cases* truly described and truly drawn his visual impressions, *as far as he has distinctly perceived them*. But on account of the faulty mode of representation he seems to have neglected one half of the visual impressions which he ought to have perceived. His papers also show misconception on some fundamental points in binocular vision which I cannot now notice. I wish now only to compare his visual results with my own, and thus to show the superiority of my mode of representation. Among his many figures I will select only two, as the principles involved are precisely the same in all.

In the accompanying figure (fig. 29) I reproduce his drawing fig. 1. Pl. I, only changing the lettering so as to bring it into accord with the previous figures of this paper. The visual result which he obtains when this drawing is viewed on the plane already described, the point of sight being at A, is given in fig. 30. In this result V is the combined visual lines vr and vl, b is the median line B of the drawing, as seen by the right eye, and b' the same line as seen by the left eye, but the right eye image of vl and the left eye image of vr are entirely overlooked.

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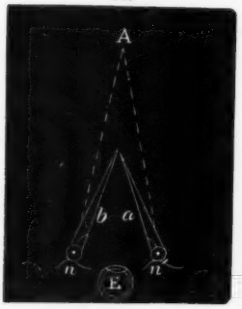

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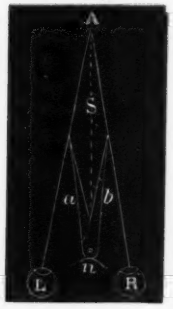

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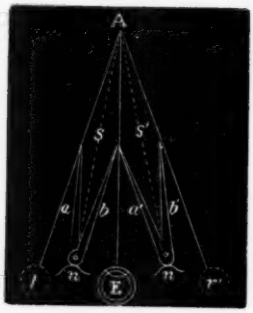

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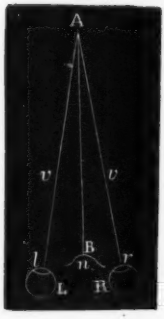

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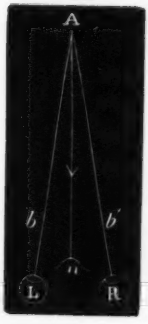

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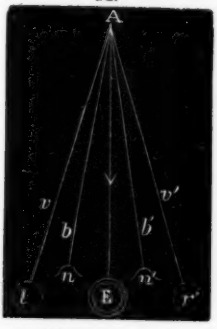

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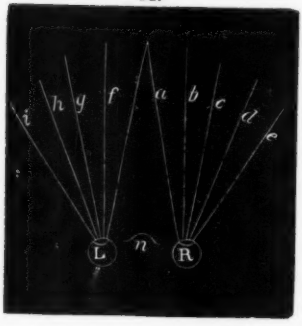

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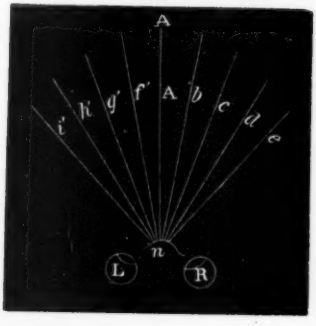

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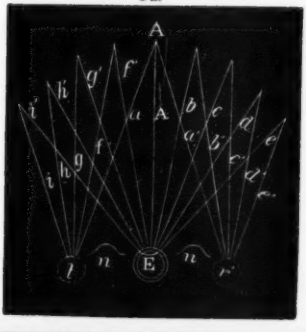


The result which I obtain is given, represented by my method, in fig. 31.

In fig. 32, I reproduce one of his more complex drawings, viz: fig. 1 of his Pl. II, lettering of course as before. The visual result which he obtains by viewing this figure with the point of sight at A is given in fig. 33, where A *n* is the combined lines A R and A L of the drawing, *b c d e*. The corresponding lines of the drawing, as seen by the right eye alone, and *f' g' h' i'* corresponding lines of the drawing as seen by the left eye alone. My result is given in fig. 34. By careful comparison of these two results, remembering that capitals represent combined images, small italics right-eye images, and dashed italics left-eye images, the nature of their differences will be readily understood. Mr. Townes in his result makes the right eye shift to the left *only the right portion*, and the left shift to the right *only the left portion*, of the drawing. My result shows the *whole drawing* shifted by the right eye to the left and the left eye to the right. Mr. Townes has neglected entirely the left eye images of *b c d e*, and the right eye images of *f g h i*. Every one of his visual results of figures drawn on the plane described, except a few in which he used a median septum, are faulty in the same way, and may be reconstructed on the same principle, with the utmost certainty, even before performing the experiment.

Oakland, Cal., Sept. 27, 1870.

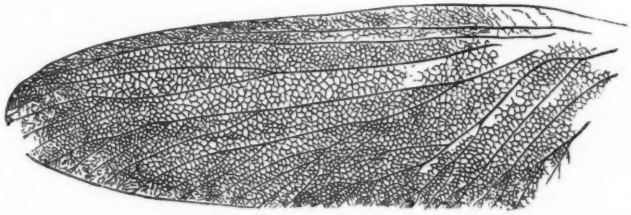
ART. IX.—*Brief Contributions to Zoölogy from the Museum of Yale College.* No. IX.—*Notice of a Fossil Insect from the Carboniferous formation of Indiana;* by SIDNEY I. SMITH.

Paolia vetusta, gen. et sp. nov.

THE fossil insect wing, represented in the accompanying figure, was sent for examination and description, from the Museum of Hanover College, Indiana, by Prof. E. Thompson Nelson. It occurred in the "grit" quarry near Paoli, Orange county, Indiana, and was discovered by the workmen while sawing the stone into small pieces for whetstones. The fossil is remarkably perfect, nearly the entire wing being beautifully preserved, and of peculiar interest for showing the complete wing-skeleton of an insect so ancient as the coal period. It is apparently the anterior wing of the left side, and measures 2·54 in. in length and about ·85 in. in breadth, the posterior margin being slightly cut away by the saw.

Along the anterior border, the space between the second and third nervures is bent abruptly downward in the specimen, so as to be somewhat foreshortened as seen in the figure. This

is probably a natural fold such as is found in many Neuropterous insects.



The neuration is remarkable for the number of slender branchlets which the nervures throw off toward the posterior border and the tip of the wing. The first, or marginal, nervure* is continuous to the tip of the wing. The second is simple and straight nearly to the extremity, where it throws off several very minute branchlets and itself impinges upon the margin, about three-fourths of an inch from the tip of the wing, as a very delicate nervure. The third divides a short distance from its origin; the anterior branch runs parallel to the second nervure, is separated from it nearly as far as the second is from the first, and is simple to near its extremity, where it is twice forked, the minute branchlets reaching the margin a little way from the tip of the wing; the lower branch is broken and wanting just at its origin; it forks about the middle of its course, the anterior of these secondary branches again subdividing like the anterior primary branch, while the posterior secondary branch divides about the middle of its course, the anterior of these tertiary branches again dividing near its base, and the anterior of the branchlets reaching the margin at the apex of the wing. The fourth nervure divides a third of the way from the base to the tip of the wing; both these branches fork or branch several times, so that there are twelve branchlets extending from them to the margin, and one from the posterior, or inner, side which is lost in the space between the fourth and fifth nervures and does not reach the margin. The fifth divides near the base of the wing; the inner branch runs an almost straight course to near the margin where it divides; the outer primary branch divides about the middle of its course, both its branches again dividing or branching several times, and the first branchlet on the outer side not reaching the margin. The basal portion of

Figure 1, is once and a half natural size lineally, and was drawn on wood from a photograph.

* For convenience of comparison, I follow Heer's notation of the main nervures of the Neuropterous wing, as adopted by Mr. Scudder, regarding what are usually considered the primary branches of the median as distinct nervures, making six in all.

the sixth nervure is wanting in the specimen, but undoubtedly divides at its origin. The outer branch forks a short distance from the margin, and the outer of these branchlets, and probably the inner also, again fork very near the margin. The inner branch forks about the middle of its course, both these secondary branches forking once or twice.

All the interspaces are filled by a finely reticulated and very delicate net-work of minute nervures, but there are no straight cross-nervures. There is, however, a series of nervures running from the second nervure to the anterior margin, which are stronger than the general net-work, but still seem to be a part of it.

This wing differs so much in neuration from any family of recent insects, that it is difficult to point out any near affinity with living forms, although it shows some points of resemblance to several families of Neuroptera, and especially to the Ephemerids. To *Hemeristia* and *Miamia*, described and figured by Prof. Dana* and Mr. Scudder,† from the Carboniferous formation at Mazon creek in Illinois, it shows more resemblance, but still differs more from either of these genera, which are considered distinct families by Mr. Scudder, than they do from each other. From the forms described by Mr. Scudder in the third volume of the Geological Survey of Illinois, it is even more widely separated.

To the species described from the European Carboniferous, it seems to be still more closely allied. Prof. Hagen, to whom a tracing of the specimen was sent, is of the opinion that it will be found to belong to the same genus as the *Dictyoneura libelluloides* of Goldenberg.‡ Prof. Hagen, also, regards the remarkable *Eugereon Böckingi* Dohrn,§ which has attracted so much attention, as the same genus, and perhaps the same species, as *D. libelluloides*. In both *Dictyoneura* and *Eugereon*, as figured, the wings have considerable resemblance to the specimen from Indiana, but in neither of them are the nervures so numerously branched toward the posterior border of the wing, and in *Eugereon* the spaces between the three first nervures next the anterior border are connected by straight cross-nervures. There are also important differences in the branching of the main nervures. Regarding these differences as at least of generic value, I propose to institute a new genus for the species from Paoli, and call it *Paolia vetusta*.

* This Journal, vol. xxxvii, pp. 34, 35, figs. 1, 2, 1864.

† This Journal, vol. xl, p. 268, 1865, and Memoirs Bost. Soc Nat. Hist., vol. i, p. 173, pl. 6, 1866.

‡ Die fossilen Insecten der Kohlenformation von Saarbrücken (aus den Palæontographicis von Dunker und Meyer, Band iv, 1854), pl. 1.

§ Eugeron Böckingi, eine neue Insectenform aus dem Todtliegenden (aus Palæontographica von Dunker, Band xiii, 1866, pl. 41.

ART. X.—*Earthquake of October 20th, in Northeastern America;* by ALEX. C. TWINING.

NOTICES of the earthquake of October 20th last, collected from the newspapers and received from other sources, display an area of disturbance extending from New Brunswick in the East, to the State of Iowa in the West, and from the Lakes and the River St. Lawrence in the North, to Cincinnati and Richmond, Va., in the opposite direction. No doubt the actual area of disturbance was far more extended, especially in the East and North; for the manifestations were at least as strong in degree at the utmost bounds of our information in these last directions, as in those parts with which we are most immediately familiar. The same conclusion is confirmed by the circumstance that, even where the motions were most fully developed, they were not obvious to persons not favorably situated for receiving impressions from them. It is easy to believe that they escaped public attention over large regions where they took place sensibly, although but feebly.

At this place—New Haven, Connecticut—the movements, both in the precise time of their occurrence and in the attendant circumstances, were immediately and critically investigated by the writer, from the testimony of many intelligent citizens. Independently, and in their various situations and positions, the witnesses agreed in describing an oscillatory movement, to and fro, in the general direction of N.N.E. to S.S.W.,—not level but rocking. This last sensation is plainly indicative quite as much of a vertical displacement, combining irregularly and oppositely with the horizontal, as of any alternations of inclination in the earth's surface, after the manner of a wave, and a consequent vibratory condition of persons and objects. Still, that the latter did take place is a conclusion that seems to be made inevitable, by the invariable fact that the oscillations were by far the most perceptible in lofty situations,—for example, in the ascent progressively from the lower floors of edifices to the upper stories. In the latter, at least, objects were both felt and seen to be in motion; and suspended objects, as lamps and pictures, were set into vibration, with a range of three or four inches and, when free, in the direction already specified. There were two distinct shocks. In estimating the duration of these and of the interval between them, and also in estimating their component oscillations, these durations and intervals were in no case recorded from mere verbal statements, but means were employed to ascertain the informant's *mental* impressions and recollections, applying to them my own measurement, as to time.

These impressions and recollections would often give durations more than double, in some instances, of the like in other instances. Taking, however, the most trustworthy, and allowing to the others a weight proportioned to their value, under the circumstances, the first shock was found to have continued through about nine seconds, the second through about eleven seconds, and the interval about five,—making, altogether, twenty-six seconds. The time of each double vibration—that is both to and fro—was concluded to approximate to a second and one-third,—although, so far as mere numbers were concerned, the witnesses more generally approved not more than one second, or even less. The precise time of day will be referred to farther on. It is remarkable that motions which were not even noticed, ordinarily, by persons in basement stories or in the streets, should have produced in most of those who felt them nausea or dizziness or other affections, according to the temperament of the individual.

A careful comparison of the various newspaper paragraphs which have come to hand—nearly forty in number—makes it clear that the general phenomena were everywhere the same. Every where there were two shocks experienced of a few seconds each, and a brief interval. These are about as variously stated in duration by the newspapers as the same were stated by different observers at New Haven:—thus, at Troy 15 to 20 seconds in all, at Montpelier 30, at Cleveland 15 to 30, the prevalent authority being for the latter, at Boston 18 to 30, at Brunswick, Me., 30 to 40, at Cincinnati 30 and less, at Hartford 20 to 60, at Cornell University, Ithaca, three shocks of 15 seconds each—the *three* being too exceptional among the mass to be credited, and having but one other parallel, that is, Brooklyn. At Harvard College Observatory the duration of tremor was from 8 to 15 seconds by the estimate of different observers,—two shocks with a continuous tremor between, and the component oscillations 40 in a minute according to observer W. A. Rogers. At East Saginaw, Mich., the first shock is described as 10 seconds, then an equal interval, then a second shock of 10 seconds. A very few of the statements are so abnormal, one or two in their brevity (one or two seconds) and about as many in their length (two or three minutes), that no weight can be accorded to them. On the whole, the entire duration—26 seconds—at New Haven, is confirmed as having been about the duration in all other places observed—a uniformity probable in itself. In one instance the oscillations are spoken of as four or five in a second, and as being like the motions of a boat,—also in one other (at Keene) they are described as having been vertical.

Respecting the relative amount of disturbance at different places, the sensations experienced by individuals would be but

a fallacious test, because so much influenced by temperament, as well as by situations and positions. Persons sitting, for example, were far more sensitive than persons standing. Again, while operatives in upper rooms often rushed down—as here at New Haven—in a panic, persons standing or walking in the immediate vicinity below, did not even perceive the phenomenon. The same remark applies to the reported swaying of walls and buildings ten inches to a foot, which is no doubt exaggerated in amount. But a vague conclusion may be derived from specific occurrences, such as the displacement of objects, the cracking of window panes or of ceilings, and the falling of chimneys, as well as from the degree of public notice which the event excited. All that, in the present instance, can be rationally deduced from such indications would seem to be that the amount of disturbance at New York, at Cleveland, at Milwaukee and Detroit was about the same as at New Haven; that it was greater at Boston, at Brunswick, at Montreal and at Quebec; that it was less at Cincinnati, at Chicago, and at Dubuque, and was but just distinctly perceptible at Richmond, Va. In other words, the movement was more remarkable in the East and North, and less in the West and particularly in the South. It is also believed that no like occurrence comparable in the aggregate with this one has been experienced in New England during the present century, nor since November 18, 1755—supposing the newspaper quotation, relative to that event, made by a Boston correspondent, from "a book published in 1786" to be correct and authentic.

It only remains to compare the times noted at the various places concerning which our information is sufficiently authentic. At this place there were two among the observers who separately noted the time by reference to standard time-pieces. By each the same time of beginning was given, within a few seconds;—that is, $11^h\ 19^m\ 46^s$. The average of three other observations by time-pieces whose errors could not be ascertained except on the day after, proved to be very nearly the same. The same conclusion was farther confirmed, with sufficient exactness, by the average of three clocks which had stopped, as observed upon their dials. Prof. C. Smallwood of Montreal Observ'y has obligingly furnished observations of local time in the Canadas which are entitled to confidence, viz., at Quebec, Montreal and Owen's Sound, and also at St. Johns, N. B. These will be found in their place in the table below. The time at Boston, taken by Mr. Farmer with care, is communicated by Prof. Newton, together with most of the newspaper notices herein referred to. That at Harvard College Observatory has been obligingly furnished to the writer by Assistant Arthur Searle, together with the other facts herein stated as observed at that locality. The

time at Cleveland, Ohio, is the average of three clocks that were stopped, and is confirmed essentially by Col. Whittlesey, but without an actual reference of either to a standard time-keeper. From these combined the following table is composed; of which column 1 gives the place of observation; column 2, the latitude; column 3, the difference of local time from New Haven corresponding to the longitude; column 4, gives the respective local times corresponding to $11^h\ 19^m\ 46^s$—the beginning at New Haven; column 5, is an empirical scale of progress proportioned to the differences of longitude and the presumed differences of absolute time at St Johns and Chicago; column 6, gives the observed times of the earthquake; column 7 is comparative of the two preceding, and shows the presumed error of observation, whether too late or too early at each of the places respectively. It would obviously only obscure the discussion to take account in the table, of times which do not purport in the notices themselves to be near approximations, or which are adopted from the railroad time-pieces without supplying the proper correction, or which are found by comparison with the mass of observations to be grossly in error. To one or the other of these categories belong the notices from Brunswick and Portland, Me., Burlington, Vt., Warrensburg, Saratoga, Cooperstown, Cornell University, and Auburn, N. Y., Scranton and Titusville, Pa. The notice from Cincinnati, Ohio, is retained on account of the extreme position.

1.	2.	3.	4.	5.	6.	7.
Place of Observation.	Lat. of place.	Time diff. from New Haven.	Simultaneous local Times.	Empirical Times.	Observed Times.	Diff. early or late.
	° ′	m. s.	h. m. s.	h. m. s.	h. m.	m. s.
St. Johns, N. Bruns.	45 9	+27 34	11 47 20	11 45 5	11 45	0 5 early.
Bangor, Me.	44 48	+16 34	11 36 20	11 34 55	11 35	0 5 do.
Lewiston, Me.	44 5	+10 46	11 30 32	11 29 33	11 30	0 27 late.
Boston, Mass.	42 21	+ 7 28	11 27 14	11 26 30	11 25½	— 1 1 early.
Harv. Coll. Obs'y.	42 23	+ 7 11	11 26 57	11 26 14	11 25¾	+ 0 25 do.
Quebec, L. C.	46 49	+ 6 38	11 26 24	11 25 44	11 27	1 16 late.
Providence, R. I.	41 50	+ 6 7	11 25 53	11 25 15	11 26	0 45 do.
Norwich, Conn.	41 33	+ 3 14	11 22 50	11 22 25	11 20	2 25 early.
Montpelier, Vt.	44 17	+ 1 18	11 21 4	11 20 48	11 24	3 12 late.
Hartford, Conn.	41 46	+ 0 59	11 20 45	11 20 30	11 20¼	+ 0 4 do.
New Haven, Conn.	41 18	0 0	11 19 46	11 19 36	11 19¾	+ 0 10 do.
Montreal, L. C.	45 31	— 2 38	11 17 8	11 17 7	11 17	0 7 early.
Dudley Obs., N. Y.	42 40	— 3 20	11 16 26	11 16 31	11 15	— 1 16 + do.
Hudson, N. Y.	42 14	— 3 22	11 16 24	11 16 29	11 14	2 29 do.
Schenectady, N. Y.	42 48	— 4 0	11 15 46	11 15 54	11 15	0 54 do.
New York.	40 43	— 4 20	11 15 26	11 15 35	11 15	0 35 do.
Warsaw, N. Y.	42 43	—20 58	10 58 48	11 0 13	11 0	0 13 do.
Toronto, U. C.	42 40	—25 44	10 54 2	10 55 48	10 55	0 48 do.
Owen Sound, U. C.	44 44	—32 2	10 47 44	10 49 58	10 52	2 2 late.
Cleveland, Ohio.	41 30	—35 10	10 44 36	10 47 5	10 44½	2 25 early.
Cincinnati, Ohio.	39 6	—46 6	10 33 40	10 37 8	10 30	7 38 do.
Chicago, Ill.	42 0	—58 38	10 21 8	10 25 23	10 25	0 23 do.

From all observed times of the *end*, if expressly so given, twenty-six seconds has been deducted for the *beginning*, as above. Wherever no mention is made of the period in the shock to which the observation applies, the times stated in the notices respectively have been inserted in the table without change, as being the presumed time of the beginning.

An inspection of this table discloses no considerable north-and-south movement. A nearly east-and-west progress is that which appears most consistent with the aggregate of times. Indeed it presents itself as the only one at all consistent with a uniform and regular progress. Nor is there discovered in the table any decisive indication of a change of direction as between the east and the west of the Alleghanies; but the various accounts, so far as they go, indicate the very reverse. Thus the direction of N.E and S.W., noted at Brunswick, was accurately N. 10° E. at Harvard Observatory, while it was N.N.E. at New Haven, and almost identically the latter also at Chicago. But at Cleveland and Detroit it is described as E. and W., and at Burlington, Vt., N. and S. One account from Boston makes it, incorrectly, N.W. and S.E., and another from Hartford the same at the latter place.

The best approximate result appears to be that the earthquake made progress from about E. 6° N. to about W. 6° S., *at the rate of one hundred and sixty miles a minute*, being six minutes and a half from St. Johns to Chicago; *while surprisingly, the ordinary direction of the subordinate undulation was about N. by E.* But there was not absolute regularity of rate in any single direction. The times at Cambridge, at Albany and at New Haven compared together, disclose appreciable deviations from the general *regimen* of the table; and the same is probably true with respect to the Cleveland observation. Unfortunately the latter was not checked by comparison with a standard time-piece,—neither, as is probable, was the Owen's Sound observation. That at Cincinnati does not purport to be more than a loose approximation.

It appears by observations of Prof. Hough at Dudley Observatory, obligingly furnished me by him, that the principal shock was observed at $11^h\ 15^m$, being a few seconds in duration, but that a tremor continued for at least a minute. No doubt there exist, at various localities not heard from, many observations recorded or distinctly remembered. It is desirable yet to have them communicated to this place, or to some one elsewhere, by whom they will be made available.

In fact while this article is in press, I am furnished through the favor of H. Paton, Esq., of the Montreal Telegraph Co. at Quebec, with the following important statements by Mr. Robert McCord, the operator who made inquiries immediately after

the cessation of the earthquake at that office, and received replies from Montreal. His condensed statements are: "All perceptible motion was over here by the time I had finished telegraphing to Montreal—*do you feel earthquake.* It would take about eight seconds to telegraph the words used. The operator's immediate reply was—*no.* About fifteen seconds after, he said —*here it is.* The gentleman at Montreal afterward explained that, although he said *no*, he did experience a slight movement at the time, but was not aware that it was caused by an earthquake. He telegraphed—*here it is*—on the instant that the shock became sensibly evident. He cannot say how long the vibration lasted after that period, accurately, but is of opinion it continued for nine or ten seconds. The following further particulars be may of interest. The shock passed over this district from a northerly direction,—a rumbling sound accompanied the shock, resembling that produced by ponderous machinery,—noise and motion increased steadily for some seconds, and gradually passed away. Buildings were violently shaken. Our office being in the upper part of a high brick building, the trembling was powerfully felt—the clock in the office was stopped by it at 11 : 25, but am not certain if the time by it previous to the shock was correct. Some observers say the shock was first felt here a few seconds before 11 : 24, and lasted forty seconds —others state fifty or sixty seconds. I am of opinion it lasted sixty seconds. No two opinions agree as to the commencement and duration."

"At Bay St. Paul and Les Eboulements—places about ninety or one hundred miles northeast of Quebec—the ground opened in several places, and water was thrown up. Slight elevations of land in some places were produced. The country in the vicinity of St. Paul's Bay is of volcanic formation, and slight shocks are of frequent occurrence. A gentleman from that vicinity informs me that they had twenty or thirty slight shocks within the past two weeks. The shock on the 20th, here, did not appear to have an undulating, upheaving motion, but to partake of a tremulous nature."

"The operator at Richmond, a place ninety miles southwest of here, says the shock was just passing away there at the period when I made the inquiry of Montreal."

Mr. McCord does not refer to specific authority for the convulsive effects at Baie St. Paul and Les Eboulements, some 55 to 62 miles in a direct line from Quebec; but they are in general conformity to what has long been known to British geologists respecting the volcanic character of the region specified. They, in fact, recall and confirm the representations made at page 236, vol. xxx of this Journal, 1836, in a paper by Capt. R. H. Bonnycastle, R. En. In that region, very probably, lay the initial

spot of the disturbance. The subject, in this view of it, merits a more extended investigation, and it is hoped that the statement here brought out will induce new communications relative to the facts.

The interpretation of Mr. McCord's interesting statements relative to the telegraphic communication, seems to be as follows: He occupied the last eight seconds of the motion at Quebec with a question put to Montreal. That question, together with the answer "no," I find by careful trial at the American Telegraph office, require not less than eighteen seconds for transmission and reception. Then there are estimated fifteen seconds of interval. Then there was the second reply of not less than eight seconds; then there were nine or ten seconds to the end of the vibrations. In other words, there transpired not less than forty-two seconds between the end of the message and of the tremors at Quebec, and the end of the same at Montreal, ninety-six miles distant in longitude, and one hundred and fifty-nine in a line N. 51° E., and which in the table above had been and still remains credited with the empirical interval of thirty-nine seconds,—showing a near accordance between these and the other facts, and 137 miles per minute of east-and-west progress.

Again, the Montreal "no" was given just as the tremors there began, and after—say six seconds after—the end of the shock at Quebec; that is to say, the beginning traversed from city to city in some six seconds more than the duration of the shock. Consequently the latter, in consistency with the above, must have continued about thirty-six seconds, in place of the twenty-six seconds ascertained at New Haven. Still again, the shock at Montreal began with the "no," which may have occupied four seconds. Then there were fifteen seconds of interval; then eight of reply; then nine to ten of estimated continuance; in all thirty-six or thirty-seven seconds for the duration of the shock. The principal movement at Montreal did not come till at least fifteen seconds after the beginning; and the testimony of the Richmond operator shows that the violence of the shock there had passed, while the tremors at Quebec were still in faint continuance. Finally, it will be observed that the time of occurrence stated by Mr. McCord and others—although not confirmed by his comparison with any standard time-piece—*tends* to represent the actual occurrence at Quebec to have been before the empirical time of column 5 in the table, instead of $1^m\ 16^s$ later, as shown in column 7.

ART. XI.—*Brief Contributions to Zoölogy from the Museum of Yale College.* No. X.—*Descriptions of some imperfectly known and new Ascidians from New England;* by A. E. VERRILL.

Molgula Manhattensis Verrill. Figure 1.

Molgula Manhattensis Dekay, Report on the Natural History of New York, Mollusca, p. 259, 1843; Binney, in Gould's Invertebrata of Massachusetts, 2nd ed., p. 25, 1870, (copied from Dekay).

BODY somewhat oval, subglobular, or slightly cordate, usually somewhat compressed laterally; when crowded often slightly adhering together laterally and more or less deformed; usually adhering to eel-grass (*Zostera*) and various sea-weeds, or to the under side of stones, by the left side, or obliquely by the basal portion. The surface including the tubes is generally thickly covered with fragments of eel-grass, broken shells, particles of sand, and various kinds of debris so as to entirely conceal the surface and disguise the form; sometimes, however, in sandy regions the surface is nearly destitute of such covering. The integument is firm and thick, and when clean it is translucent and roughened throughout with minute, granule-like papillæ, which are larger and more crowded on the upper part and around the base of the tubes, where they often form small, rough, unequal verrucæ. The tubes originate on the upper side, a little apart, the distance between usually about equal to the diameter of the anal tube;* they are both rather long and diverge by curving outward from the base. The anal tube is smaller and somewhat longer than the branchial, usually about equal in length to half the diameter of the body, and tapers to the end, which is rounded and has a small square aperture, its sides, near the end and often below, are usually roughened by irregular, uneven papillæ. The branchial tube is shorter and stouter, widest at base, truncate at the end, with six, small, obtuse, rounded papillæ surrounding the orifice; outside of these there are six, short, triangular lobes; below these there is often an irregular circle of about six to twelve rougher papillæ, alternately larger and smaller, and sometimes other more irregular ones below, between the sulcations; these are, however, sometimes wanting, as in the specimen figured. In contraction the tubes form low, rough verrucæ, the anal often four-lobed, both covered with rough, irregular papillæ.

1.

The inner tunic, which is closely adherent to the outer, is smooth, pellucid, light greenish, the dark intestine and a reddish organ below it showing through.

* The figure represents them as arising nearer together than is usually the case.

In life the body and tubes are pale yellowish green or olive; the branchial orifice is sometimes surrounded within by a ring of dull reddish brown, the six lobes have each a spot of dark brown, with smaller ones between. The anal orifice is greenish yellow.

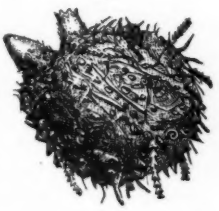
Diameter of the largest specimens seen, about 1 inch.

New York Harbor,—Dekay; Fire I., Long Island, abundant on eel-grass,—S. I. Smith; New Haven Harbor, on eel-grass, abundant,—A. E. Verrill; near New Haven Light on sea-weed, and at Savin Rock, under stones at low-water mark,—A. E. Verrill.

Molgula pannosa Verrill, sp. nov. Figure 2.

Body subglobular, or a little elongated, somewhat compressed laterally, entirely covered, except the ends of the tubes, with a firm and thick covering, composed of fragments of shells, echini, zoöphytes, worm-tubes, foraminifera, grains of sand, pieces of sea-weed, and other debris. The surface of the integument, when the foreign matter is removed, is densely covered with small granule-like papillæ, which give rise to very abundant, long, fine fibrous processes, by which the foreign matters are entangled; the basal portion of the tubes is covered with similar processes, though less numerous, which decrease toward the end, leaving the terminal portion nearly smooth. The tubes, which arise close together, are short, conical, a little divergent. The anal tube is a little longer than the branchial, swollen at base, tapering, and rounded at the end, which has a very small square aperture. The branchial tube is about the same in size, but a little shorter, subcylindrical, scarcely tapering, with six, small, prominent, acute lobes or papillæ; alternating with these are six much smaller ones. In contraction the tubes can be wholly withdrawn, and then the body looks like a more or less irregular ball of dirt.

2.



In life the color of the clean integument is a dull, dark, grayish green; the tubes lighter, or dull olive-green.

A large specimen, when expanded, was 1 inch in length; ·75 broad; ·50 thick.

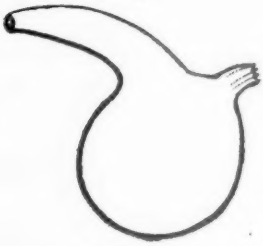
Eastport, Me., 10 to 50 fathoms, muddy and shelly bottoms,—Expeditions of 1868 and 1870; off Head Harbor, Campo Bello I., 80 fathoms, muddy,—Expedition of 1868.*

* The expeditions of 1859 and 1863 were by the author; that of 1864 by the author and S. I. Smith; that of 1868 by A. E. Verrill, S. I. Smith, H. E. Webster and Geo. A. Jackson; that of 1870 by A. E. Verrill, Oscar Harger and C. H. Dwiuelle.

Molgula retortiformis Verrill, sp. nov. Figure 3.

Body subglobular or more or less oval, generally higher than broad and a little compressed laterally, destitute of a distinct disk, though generally adhering. The integument is thick and firm, but translucent, usually covered, together with the tubes, by various zoöphytes, ascidians, fragments of shells, etc., which form a coarse, rough, and rather loosely adherent coating, but this is usually incomplete or thin and the surface more or less exposed. When cleaned the surface is roughened by small, scattered, granule-like papillæ, and irregular rough processes, while in large specimens it is more or less rudely wrinkled. The large tubes arise from the upper part, a short distance apart, and are widely divergent and very unequal. The anal tube is much the longest, usually equalling in length the diameter of the body, very large and swollen at the base, curving outward and tapering to the end, which is blunt, with a small square aperture, its surface is often roughened with irregular processes, and in partial contraction is often sulcated. The branchial tube is usually about one-fourth as long, much smaller at base, curved outward, the aperture surrounded by six, short, conical, acute papillæ or tentacular lobes, its surface below often rough with irregular verrucæ. In contraction the tubes form large, low and rough verrucæ, or may be wholly retracted and invaginated, together with a portion of the integument of the body around their bases.

3.



When living the color is uniform light olive, or grayish green.

The larger specimens are often 1·50 inches in diameter; with the anal tube nearly 2 inches long; the branchial about ·50. In the collection there are specimens of all sizes from this down to less than ·25 of an inch in diameter.

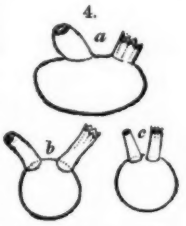
The young have essentially the same characters as the adults, but are smoother.

Eastport Harbor, South Bay, etc., from extreme low-water of spring tides to 50 fathoms, on rocky and shelly bottoms, adhering loosely by the sides, to other ascidians, zoöphytes, shells, etc., not abundant, though often met with,—Expeditions of 1864, '68, and '70; off Head Harbor, 80 to 120 fathoms,—Exp. 1870.

Molgula littoralis Verrill, sp. nov. Figure 4, a.

Body transversely oblong-oval, somewhat flattened laterally, and attached obliquely by a part of one side (usually the right)

to the under side of stones; when young subglobular. Integument firm and tough, but translucent, the tubes similar to the body; the surface smoothish in young specimens, in older ones slightly wrinkled, not fibrous, but somewhat roughened with sparse granule-like papillæ, to which a thin, imperfect coating of foreign substances is sometimes attached; in contraction the surface is reticulated with wrinkles, with more elevated interstices. Tubes short and stout, arising from the upper side, and separated by a distance about equal to or exceeding their diameters, quite divergent, the anal tube most so, not wholly retractile. The anal tube is about twice as long as the branchial and usually swollen in the middle, rounded at the end, with a simple square orifice, which in full expansion often becomes roundish. The branchial tube has about the same diameter, and is short, cylindrical, with six short conical papillæ around the orifice. In contraction the tubes form low verrucæ.



When living the color of the body is translucent, dull greenish or olive, between the tubes usually more or less ferruginous brown, or russet; on the sides the viscera show through as dull orange-colored and darker blotches.

One of the larger specimens was, while living, ·45 of an inch long; height ·35; length of branchial tube ·10; diameter ·10; distance between bases of tubes ·08.

Eastport, Me., under stones near low-water mark, not uncommon,—Expeditions of 1868 and 1870.

In Binney's edition of Gould's Invertebrata of Massachusetts, there are three figures, reproduced from Mr. Burkhardt's drawings, which Mr. Binney refers somewhat doubtfully to *Glandula mollis* Stimpson, neither of which can represent that species. Of these, Fig. 317 (Pl. xxii) may, perhaps, represent the present species, but the apertures show no details.

Molgula papillosa Verrill, sp. nov. Figure 4, *b*.

Body free, nearly globular, or transversely suboval, usually slightly compressed laterally. Integument rather thin, translucent, the surface, both of the tubes and body, entirely covered by particles of sand, broken shells, foraminifera, etc., which adhere firmly. When cleaned the whole surface is thickly covered with prominent granule-like papillæ and numerous slender fibrous processes; the granules are most conspicuous on the tubes, where they usually have a rusty color. The tubes are long, subequal, and their bases are separated by a space usually greater than their diameters; they are quite divergent, both of them curving outward, the anal tube most abruptly. The branchial tube is cylindrical, somewhat longer than the

anal, equal to or exceeding the diameter of the body, the orifice surrounded by six, rather long and slender, conical, divergent papillæ. The anal tube often bends suddenly outward, tapers slightly, and has a small square aperture, surrounded by a circle of dull reddish brown. In contraction the tubes are not retracted, but are usually shortened to about one-half their length. In life the body, when cleaned, is pale grayish, with an almost transparent integument, through which the convolutions of the dark intestine are conspicuous.

The largest specimens are about ·40 of an inch in diameter.

Eastport, at low-water mark under stones, and off the northern side of Treat's Island, in 10 to 20 fathoms, shelly bottom,—Expedition of 1870.

Molgula pilularis Verrill, sp. nov. Figure 4, *c*.

Body unattached, globular, covered with a thin layer of mud, and when the tubes are retracted, looking like a small soft ball. Integument of the body, when cleaned, very thin, soft, nearly transparent, thickly covered with minute granules, and minutely fibrous, usually concealed by the adhering particles of mud and fine sand, but this can be easily removed. The tubes are naked, smooth, nearly transparent, subconical, slender, nearly as long as the diameter of the body, originating close together, and but slightly divergent, both of them nearly straight; they can be wholly retracted and their bases are surrounded and connected by a narrow, naked, oval or oblong band, which is usually conspicuous when the tubes are withdrawn; in partial contraction, the tubes are conical, subpellucid, reticulated with white lines. The branchial tube is a little shorter than the anal, the aperture surrounded by six, acute, conical papillæ, and twelve, small, dark brownish spots. Anal tube a little smaller, slightly longer, a little tapering, with a small square aperture, surrounded by four small lobes and four small, reddish brown eye-spots.

In life the body, when cleaned, is transparent grayish, the dark intestine showing through very distinctly; tubes greenish at base.

Diameter usually about ·20 of an inch, seldom more than ·25 or ·30.

Eastport Harbor, in 10 to 20 fathoms, muddy, common,—Expeditions of 1864, '68, '70; South Bay, Lubec, in 6 fathoms, soft mud, very abundant,—Exp. of 1868; Grand Menan, off Swallow's Tail, 15 to 20 fathoms, soft mud,—Exp. of 1870.

Cynthia glutinans Möller (Kroyer's Naturhistorisk Tidsskrift, iv, p. 94, 1842) seems to resemble this species in form and appearance, but if it belongs to *Cynthia*, or to *Glandula* where Stimpson refers it, both apertures must be square. *G. mollis* Stimp. must be a closely allied species, if not identical.

(To be continued.)

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Thermo-chemical investigations.*—Under this title J. Thomsen in Copenhagen, has published a series of valuable researches, the principal results of which, in the author's own language, are as follows:

(1.) As a general law it appears that for all acids

a. When a molecule of sodic hydrate in aqueous solution acts upon an acid the evolution of heat is very nearly proportional to the quantity of acid until this amounts to 1, $\frac{1}{2}$, $\frac{1}{3}$ or $\frac{1}{4}$ of a molecule, according as the acid is one-, two-, three-, or four-basic.

b. But when the quantity of acid exceeds that which is necessary to form the normal salt, a difference between the acids is exhibited, so that according to the constitution of the acid the heat* produced by its excess is either zero, positive or negative.

(2.) *a.* When a molecule of an acid in aqueous solution acts upon sodic hydrate the evolution of heat in a majority of cases is approximately proportional to the quantity of soda until this amounts to 1, 2, 3 or 4 molecules of sodic hydrate according as the acid is one-, two-, three-, or four-basic. The deviations from this law find an explanation in what is stated under heads 1—6.

b. But if the quantity of soda is increased above this, no material change occurs in the heat-toning.

(3.) If the magnitude of the molecule is determined in some other manner, the basicity of the acid, if indeed it has such, may be certainly found by this method of investigation.

(4.) From this it distinctly appears that sulphydric acid is monobasic and that its formula must be considered H.SH; since the second atom of hydrogen cannot in an aqueous solution be replaced by sodium. This result which at first sight appears strange is seen upon closer consideration to correspond completely with the more recent chemical theories, since on the one hand it perfectly explains the behavior of the acid, and on the other presents a material support to the theory of hydroxyl, as the great analogy between sulphur and oxygen renders it extremely probable that the formula of water must then be H.ΘH, and that water is to be regarded as a mono-basic acid. It follows also that the so-called neutral metallic sulphides, e. g., Na_2S, do not exist in an aqueous solution, any more than the corresponding oxides ($Na_2\Theta$), and that the so-called sulphydrates, Na.SH, Ba.2SH, and Mg.2SH, are to be regarded as normal salts of the acid soluble in water.

(5.) Since the mono-basic acids contain only one atom of hydrogen replaceable by sodium, the second molecule of sodic hydrate must be without action upon the sodium salt, which is fully verified by experiment. Since further the mono-basic acids form no acid salts, an excess of acid must be without action upon the sodium

* In the original "Wärmetönung," *heat-toning.*

salt, which is also verified by experiment. In strictness the second molecule of acid produces almost always a slight *toning* of the heat, generally an absorption of heat, but this amounts to only a couple of thousandths of the heat of neutralization. In the case of fluohydric acid we find an exception, since the second molecule of acid produces an absorption of two per cent of the heat of neutralization. This fact and the existence of the acid fluoride of sodium make it doubtful whether fluohydric acid belongs to the mono-basic acids. The numbers for meta-phosphoric and hypophosphorous acid also exhibit a slight toning of the heat (about 1 pr. ct.) for the second molecule of acid. But since the numbers for the first acid cannot be sharply determined in consequence of its great liability to change, and since the number for two molecules of the second were accidentally determined by only one experiment, I lay no stress on this deviation from the rule. In the case of fluohydric acid, however, the deviation is fully established.

(6.) The bibasic acids which contain two atoms of hydrogen replaceable by sodium may be divided into several groups according to the quantity of heat which the first and second molecules of sodic hydrate evolve.

The first group embraces the bibasic hydrogen acids—fluosilicohydric acid, SiF_6H_2, and chlorplatinhydric acid, $PtCl_6H_2$; in these there is no noteworthy difference in the quantity of heat which the first and the second molecule of sodic hydrate produces; they behave therefore like the monobasic hydrogen acids, only the evolution of heat for one molecule of acid increases proportionably to the quantity of soda, up to the second molecule of sodic hydrate.

The second group includes sulphuric, selenic, oxalic and tartaric acids; in the neutralization of these acids the first molecule of sodic hydrate produces a less degree of heat than the second. The difference amounts to 5 to 1800 units.*

Sodic hydrate.	Sulph. acid.	Selenic acid.	Oxalic acid.	Tartaric acid.
1. Molecule,	146 ..	148 ..	138 ..	124 ..
2. Molecule,	164 ..	156 ..	145 ..	129 ..

The third group embraces sulphurous, selenious, carbonic and boric acids. In the neutralization of these acids the first molecule of sodic hydrate produces a greater degree of heat than the second: the difference amounts to from 18 .. to 28 ..

Sodic hyd.	Sulphurous acid.	Selenious acid.	Carbonic acid.	Boric acid.
1. Molecule,	159 ..	148 ..	110 ..	111 ..
2. Molecule,	131 ..	122 ..	92 ..	89 ..

In this group belong chromic acid, phosphorous acid and probably also succinic acid. In the reaction of the normal salts of the acids of the second group upon the corresponding acids an absorption of heat occurs, as is easily seen, while the normal salts of the

* Thomsen omits zeros and figures less than 100, and writes two dots only after the significant figures.

acids of the third group produce in the corresponding reaction an evolution of heat.

(7.) The tribasic acids exhibit differences similar to those of the bibasic acids, citric acid belonging to the second group, arsenic and orthophosphoric acid to the third. Thus we have

	Sodic hydrate.	Citric acid.	Arsenic acid.	Ortho-phosphoric acid.
1.	Molecule,	124 ..	150 ..	148 ..
2.	Molecule,	126 ..	126 ..	123 ..
3.	Molecule,	132 ..	83 ..	69 ..

Hence the second or third molecule of sodic hydrate evolves with citric acid *more*, with arsenic and ortho-phosphoric acid less heat than the first molecule. The difference in the evolution of heat in the second and third molecule of sodic hydrate is besides very large for arsenic and phosphoric acids.

(8.) The three groups which I have established in the class of bibasic acids, and of which two are found also in the class of tribasic acids, have their probable origin in the different constitution of the acids. In fact in the case of a bibasic acid the position of the two atoms of hydrogen replaceable by sodium may be conceived in three different ways. Thus sulphuric acid may be expressed by the following three formulas:

$$S\Theta_4 \left\{ \begin{matrix} H \\ H \end{matrix} \right., \quad S\Theta_3 \left\{ \begin{matrix} \Theta H \\ H \end{matrix} \right., \quad S\Theta_2 \left\{ \begin{matrix} \Theta H \\ \Theta H \end{matrix} \right.$$

The first formula might be most probable if the acid were a hydrogen acid; the second is most probable for an acid whose anhydrid exhibits a slight affinity for water; the third is the formula of a bibasic hydroxylic acid. The constitution expressed by the first formula probably corresponds to that of a bibasic hydrogen acid, as for instance fluosilicohydric and chlorplatinhydric acid.

$$Si\,F_6 \left\{ \begin{matrix} H \\ H \end{matrix} \right. \quad Pt\,Cl_6 \left\{ \begin{matrix} H \\ H \end{matrix} \right.$$

Sulphurous, selenious, carbonic, chromic and boric acids probably correspond to the second formula. Of these the first four at least easily and spontaneously split into anhydrid and water, so that the hydrates (acids proper) of several can scarcely be prepared at all. We have therefore for the rational formulas of these acids,

$$S\Theta_2 \cdot \begin{matrix} \Theta H \\ H \end{matrix} \quad Se\Theta_2 \cdot \begin{matrix} \Theta H \\ H \end{matrix} \quad C\Theta_2 \cdot \begin{matrix} \Theta H \\ H \end{matrix}, \text{ \&c.}$$

This corresponds entirely with the behavior of the homologues of carbonic acid, like glycolic acid and the other members of the lactic acid series, which are probably diatomic, but usually appear monobasic, as the second atom cannot be easily replaced by sodium, but readily by alcoholic and acid radicals. To the third formula correspond sulphuric, selenic, oxalic and tartaric acids, the formulas of which according with the usual assumption, become as follows:

$$S\Theta_2 \cdot \begin{matrix} \Theta H \\ \Theta H \end{matrix}, \quad Se\Theta_2 \cdot \begin{matrix} \Theta H \\ \Theta H \end{matrix}, \quad C_2\Theta_2 \cdot \begin{matrix} \Theta H \\ \Theta H \end{matrix},$$

The analogy of the tribasic and bibasic acids leads for citric acid to the formula, $C_6H_5O_4 . 3OH$, while the formulas for the three modifications of phosphoric acid would be the following:

$$HPO_3 \cdot \frac{OH}{H}, \quad 2HPO_3 \cdot \frac{OH}{H}, \quad HPO_3,$$

by which the thermic difference between citric acid and the acids of phosphorus and arsenic may be explained in harmony with the prevalent chemical theories.

(9.) Silicic acid is given in the tables (accompanying this paper) as a bibasic acid, but differs materially from the other acids of this group, by being absolutely without any definite point of neutralization. The numbers of table II show, it is true, that the heat which a molecule of the acid evolves with an increasing quantity of soda, increases only very slightly when this exceeds two molecules of sodic hydrate, and table I shows that the evolution of heat increases approximately in proportion to the quantity of acid, until this amounts to $\frac{1}{2}$ molecule of silicic acid for one molecule of sodic hydrate. But table II shows at the same time that the numbers by no means increase in proportion to the quantity of soda, and table I also shows that the evolution of heat increases very materially for further additions of silicic acid. From what was shown in Section IV (Pogg. Ann., Bd. 137, p. 203), it appears that the maximum of heat which a molecule of sodic hydrate can evolve with silicic acid, and which occurs only when the quantity of acid is infinitely great, amounts to 134.. while for $\frac{1}{2}$ molecule of silica it is only 26..; further, that the maximum of heat which is evolved by the action of a molecule of silica upon sodic hydrate, and which also occurs when the quantity of sodic hydrate becomes infinitely great, amounts to only 63.. while two molecules of sodic hydrate already evolve 52... .

(10.) The anomaly in the neutralization of silica has very probably its cause in the simultaneous action of water and silica upon sodic hydrate. According to what has been said under 4, water is to be regarded as a monobasic acid, and sodic hydrate as its sodium salt. If now the sodium salt is attacked simultaneously by the two acids (water and silica), the base divides itself between the two acids in a ratio which depends upon the avidity of the acids and their quantity, (see Sect. 1, Pogg. Ann., Bd. 138, p. 94). If now the avidity of the water for the base is very small in comparison with that of the acid, this decomposes an approximately equivalent quantity of sodic hydrate, and the evolution of heat becomes therefore approximately proportional to the quantity of acid, as is also approximately the case with all other acids. If, on the contrary, the avidity of the water for the base is a quantity which cannot be neglected in comparison with that of the acid, the proportionality in the evolution of heat ceases, and then follows a law which holds good for partial decomposition, (see reference cited). The absorption of heat which takes place when a solution of sodic silicate (and also various other saline solutions) is diluted with water, has probably its cause partly in a partial decomposition of the salt by the water.

(11.) The quantities of heat evolved in the reaction of one molecule of sodic hydrate with one molecule of acid-hydrate in aqueous solution are very different. Fluohydric acid gives the greatest amount of heat (163..); then comes sulphurous acid (159..), hypophosphorous acid (152..), arsenic acid (150..); the different phosphoric acids, phosphorous, selenious, selenic and sulphuric acids give between 148.. and 144.. The evolution of heat is less in the cases of the hydrogen acids of chlorine, bromine and iodine and nitric acid (137..); much less in the cases of boric and carbonic acids (110.. to 111..) while sulphydric, silicic and cyanhydric acids give the smallest amounts of heat. If however we compare the evolution of heat which a molecule of sodic hydrate produces with the quantity of acid necessary to form a normal salt, the order of the series is somewhat different, but here also fluohydric acid occurs with the greatest quantity of heat (163..); then follow sulphuric, selenic and hypophosphorous acids (155.. to 152..), then sulphurous, hyposulphuric, phosphorous and oxalic acids (145.. to 141..), and so we pass gradually down to sulphydric, cyanhydric and silicic acids.

(12.) For some of the acids which I have studied the heat of neutralization had been determined already. The older investigations often show material differences from the numbers determined by me. The determinations of Favre and Silbermann in particular differ greatly. The results of these investigators for chlor-, brom- and iodhydric acids and for nitric and phosphoric acids are from 10 to 12 per cent too high, for instance for the first four acids 151.. to 152.. instead of 137... The cause very probably lies in the inaccurate indications of the mercurial calorimeter employed by them, and I doubt very much whether the experiments recently made with the same apparatus possess a greater accuracy. I have already found several material errors in the published results to which I shall return hereafter. For the rest I refer, with reference to the inaccuracy of the results obtained with mercury, to my communication in the Reports of the German Chemical Society at Berlin, 1869, p. 701. W. G.

II. Geology and Natural History.

1. *Notes on the American Mastodon and other fossils;* by Dr. J. Leidy (Proc. Acad. Nat. Sci. Philad., Sept. 1870).—Dr. Leidy, after brief notes on the specimens of Mastodon in Boston and Cambridge, makes the following observations on some bones in the Museum of Amherst College.

Prof. Shepard has recently collected together many interesting fossil remains of vertebrates. Among these are a multitude of specimens obtained by his son from St. Helena Island, and the famous Ashley River deposits of South Carolina. Those from the latter locality consist mainly of Zeuglodons, Cetaceans and Fishes, but also include remains of Mastodon, the Elephant, and of *Equus Major* and *E. fraternus*. The St. Helena Island fossils consist of bones, fragments of jaws and teeth of the Mastodon.

Among them were noticed two inferior tusks, which measured about ten inches in length and two inches in diameter at the base.

The same collection contained a large molar of the American Elephant, of the coarse plated variety, from California. Some remains of Mastodon from the latter place struck me from their peculiarity, and these Prof. Shepard was so kind as to loan to me for examination and description.

One of the specimens, which lies on the table, is the fragment of a tusk from "Dry Creek," Stanislaus Co., California. It indicates a species totally different from the American Mastodon, and in its peculiarities exhibits a relationship with the *Mastodon angustidens* of the middle tertiary period of Europe. The fragment is six inches in length, is slightly curved in two directions, and in transverse section is ovate with the anterior pole acute. The pulp cavity, opening half the diameter at the broken base of the specimen, extends about half its length to the end. The convex side of the tusk possesses, as in *Mastodon angustidens*, a broad band of enamel, which reaches from the acute edge more than two-thirds the depth of the surface. The enamel is somewhat rugose and is two-thirds of a line thick. At one spot, toward the smaller end of the fragment, it has been irregularly worn through for the extent of about an inch and a half. The opposite side of the specimen, from the acute edge, has been worn off to an extent about equal to two-fifths of the surface. The broken ends of the fragment exhibit very conspicuously the beautiful arrangement of decussating curved lines so characteristic of the ivory in the tusks of the great proboscidians.

The vertical diameter of the base of the fragment is 28 lines, the transverse diameter 19 lines; the vertical diameter at the opposite end is 22 lines, the transverse diameter 16 lines. The entire length of the tusk appears to have been less then two feet.

The question arises as to what species the tusk fragment shall be attributed. It certainly does not belong to the common American Mastodon, nor is it probable that it belonged to the pliocene *Mastodon mirificus*. May it probably pertain to the hardly known *Mastodon obscurus?* In the present uncertainty I would look on the specimen as characteristic of a peculiar species allied to the *M. angustidens* of Europe. For the name of the species I would propose that of *Mastodon Shepardi*, in honor of Prof. C. U. Shepard, whose name has so long been identified with the interests of natural history.

The second specimen, exhibited to the members, consists of a fragment of a lower jaw containing the last molar tooth, and was discovered in Contra Costa county, California. No information in regard to the age of the deposit, or the character of the locality in which the fossil was found, accompanies it. The bone is friable, and measures, below the position of the tooth, five and a half inches in depth. Attached to the fossil there is a portion of soft gray rock, part of the matrix in which it has been imbedded. The tooth is perfect and well preserved. It has the same general form and constitution as the corresponding tooth of the American

Mastodon, but is considerably smaller. It bears sufficient resemblance to the plaster cast (represented in fig. 14, pl. xxvii., of "The Extinct Mammalian Fauna of Dakota and Nebraska, &c."). of a tooth, the original of which is lost, from a miocene formation of Maryland, to be viewed as pertaining to the same species. This I had named *Mastodon obscurus.*

The crown of the tooth consists of four transverse divisions together with the merest trace of a heel. As in the cast of the Maryland tooth, the inner lobes of the crown of the California tooth are more mammillary, and less angular than in *M. Americanus.* The outer lobes, likewise as in the Maryland tooth, have better developed offsets fore and aft internally than in the latter, giving rise to a greater degree of obstruction of the transverse valleys of the crown than in the American Mastodon. The fourth division of the crown is proportionately less well developed, in comparison with those in advance, than in the latter, agreeing also in this respect with the Maryland tooth. The outer lobe of this division is formed of a pair of connate mammillary tubercles, as in the latter, but the tubercles are more equally developed. The inner lobe is a single mammillary eminence not more than half the elevation of the outer lobe. In the Maryland tooth, the corresponding lobe resembles the outer one, consisting of a connate pair of tubercles as well developed as in the outer lobe. The heel in the California tooth, as in the Maryland tooth is formed by a short mammillary eminence occupying the angular space posteriorly of the lobes of the fourth division of the crown. A basal ridge is better developed externally in the California than in the Maryland tooth.

Comparative measurements of the California tooth, with the cast of the Maryland tooth, and one of the *Mastodon Americanus* are as follows:

	California tooth.	Maryland tooth.	Mastodon Americanus.
Fore and aft diameter of the crown of the last molar tooth,	6 in. 4 lines.	6 in. 4 lines.*	7 in. 3 lines.
Transverse diameter of do.,	2 in. 9 lines.	2 in. 9 lines.	3 in. 4 lines.
Depth of do. internally,	2 in. 2 lines.	2 in. 2 lines.	3 in. 0 lines.

It is not improbable that the California tooth may have pertained to the same species as the fragment of tusk previously noticed, and, perhaps these, together with the Maryland tooth, and others previously referred to *Mastodon obscurus*, may likewise belong to the same animal. The positive determination of this question must be left for the discovery of additional material to throw light on the relationship of the different specimens which have been thus far presented to our notice.

2. *On the cause of the Motion of Glaciers;* by J. CROLL, of the Geol. Survey of Scotland, (Phil. Mag., Sept., 1870).—Mr. Croll closes his article on the cause of the motion of glaciers with the following—*on the present state of the question*, and on the *alleged limit to the thickness of a glacier.*

* Partially estimated, as the specimen is imperfect at its fore part.

The present state of the question.—The condition which the perplexing question of the cause of the descent of glaciers has now reached seems to be something like the following. The ice of a glacier is not in a soft and plastic state, but is solid, hard, brittle, and unyielding. It nevertheless behaves in some respects in a manner very like what a soft and plastic substance would do if placed in similar circumstances, inasmuch as it accommodates itself to all the inequalities of the channel in which it moves. The ice of the glacier, though hard and solid, moves with a differential motion; the particles of the ice are displaced over each other, or, in other words, the ice shears as it descends. It had been concluded that the mere weight of the glacier was sufficient to shear the ice. Canon Moseley has investigated this point, and shown that it is not. He has found that for a glacier to shear in the way that it is supposed to do, it would require a force some thirty or forty times as great as the weight of the glacier. Consequently, for the glacier to descend, a force in addition to that of gravitation is required. What, then, is this force? It is found that the rate at which the glacier descends depends upon the amount of heat which it is receiving. This shows that the motion of the glacier is in some way or other dependent upon heat. Is heat, then, the force we are in search of? The answer to this, of course, is, since heat is a force necessarily required, we have no right to assume any other till we see whether or not heat will suffice. In what way, then, does heat aid gravitation in the descent of the glacier? In what way does heat assist gravitation in the shearing of the ice? There are two ways whereby we may conceive the thing to be done: the heat may assist gravitation to shear, by pressing the ice forward, or it may assist gravitation by diminishing the cohesion of the particles, and thus allowing gravitation to produce motion which it otherwise could not produce. Every attempt which has yet been made to explain how heat can act as a force in pushing the ice forward, has failed. The fact that heat cannot expand the ice of the glacier may be regarded as a sufficient proof that it does not act as a force impelling the glacier forward; and we are thus obliged to turn our attention to the other conception, viz., that heat assists gravitation to shear the ice, not by direct pressure, but by diminishing the cohesive force of the particles, so as to enable gravitation to push the one past the other. But how is this done? Does heat diminish the cohesion by acting as an expansive force in separating the particles? Heat cannot do this, because it cannot expand the ice of the glacier; and besides, were it to do this, it would destroy the solid and firm character of the ice, and the ice of the glacier would not then, as a mass, possess the great amount of shearing-force which observation and experiment show that it does. In short it is because the particles of the ice are so firmly fixed together at the time that the glacier is descending, that we are obliged to call in the aid of some other force in addition to the weight of the glacier to shear the ice. Heat does not cause displacement of the particles by making the ice soft and plastic; for we know that the ice of the glacier is not soft and plastic, but hard and brittle. The shearing-force of the ice of

the moving glacier is found to be by at least from thirty to forty times too great to permit of the ice being sheared by the mere force of gravitation; how, then, is it that gravitation, without the direct assistance of any other force, can manage to shear the ice? Or to put the question under another form: heat does not reduce the shearing force of the ice of a glacier to something like 1·3193 lb. per square inch of surface, the unit required by Mr. Moseley to enable a glacier to shear by its weight; the shearing-force of the ice, notwithstanding all the heat received, still remains at about 75 lbs.; how, then, can the glacier shear without any other force than its own weight pushing it forward? This is the fundamental question; and the true answer to it must reveal the mystery of glacier-motion. We are compelled in the present state of the problem to admit that glaciers do descend with a differential motion without any other force than their own weight pushing them forward; and yet the shearing-force of the ice is actually found to be thirty or forty times the maximum that would permit of the glacier shearing by its weight only. The explanation of this apparent paradox will remove all our difficulties in reference to the cause of the descent of glaciers.

There seems to be but one explanation (and it is a very obvious one), viz., that the motion of the glacier is *molecular*. The ice descends molecule by molecule. The ice of a glacier is in the hard crystalline state, but it does not descend in this state. Gravitation is a constantly acting force; if a particle of the ice lose its shearing-force, though but for the moment, it will descend by its weight alone. But a particle of the ice will lose its shearing-force for a moment if the particle loses its crystalline state for the moment. The passage of heat through ice, whether by conduction or by radiation, in all probability is a molecular process; that is, the form of energy termed heat is transmitted from molecule to molecule of the ice. A particle takes the energy from its neighbor A on the one side and hands it over to its neighbor B on the opposite side. But the particle must be in a different state at the moment it is in possession of the energy from what it was before it received it from A, and from what it will be after it has handed it over to B. Before it became possessed of the energy, it was in the crystalline state—it was ice; and after it loses possession of the energy it will be ice; but at the moment that it is in possession of the passing energy is it in the crystalline or icy state? If we assume that it is not, but that in becoming possessed of the energy, it loses its crystalline form and for the moment becomes water, all our difficulties regarding the cause of the motion of glaciers are removed.* We know that the ice of a glacier in the mass cannot become possessed of energy in the form of heat without becoming fluid; may not the same thing hold true of the ice particle?

The alleged limit to the thickness of a glacier.—In his memoir, "On the Mechanical Properties of Ice," published in the Philosophical Magazine for January, 1870, Canon Moseley arrives at a conclusion in regard to the crushing of ice to which I am unable, without some qualifications, to agree. In his experiments ice was

* See Phil. Mag. for March 1869, p. 201.

crushed under a pressure of 308·4 lbs. on the square inch, and he concludes that if a glacier is over 710 feet in thickness the ice at the under surface must be crushed by the incumbent weight. Professor Philips also made some experiments on the crushing of ice, and he came to the conclusion that the height of a crushing column of ice is between 1000 and 1500 feet, and concluded also that if a glacier were to exceed this in thickness the ice would lose its solidity.* Whether the height of a crushing column of ice be 710, or 1000, or 1500 feet is of no consequence whatever as regards the possible thickness of a glacier. No doubt a piece of ice solidified not under pressure would be crushed to powder were it placed under a glacier 1000 feet in thickness or so; but after being crushed it would resolidify, and would then probably be able to sustain a pressure of 2000 feet of ice. This follows as a necessary consequence from the property of regelation. There is as yet, so far as I am aware, no known limit to the amount of pressure which ice may sustain. There probably is a limit; but what that limit is has not yet been determined. Canon Mosely says that "there is no glacier alleged to have so great a depth as 710 feet." The Humboldt glacier in North Greenland, according to Dr. Kane, has a depth of more than three times 710 feet. And Dr. Hayes found in Baffin's Bay icebergs (which are just pieces broken off the ends of glaciers) aground in about half a mile of water. And on the antarctic continent we have reasons for believing that the ice is in some places over a mile in thickness.†

3. *On Eozoon Canadense;* by Prof. Wm. King, S.C.D., and Thos. H. Rowney, Ph.D., of the Queen's Univ. in Ireland, and the Queen's College, Galway. 42 pp. 8vo, with three colored plates, (Proc. R. Irish Acad., July 12, 1869). Professors King and Rowney have here discussed anew the nature of the so-called Eozoon, with many additional observations, and argue for their former conclusion, that the Eozoon is of mineral origin alone. The closing paragraph of the memoir, here cited, gives some idea of their view.

Finally, to subscribe to the organic origin of "*Eozoon*," the *chemist* must become a believer in *quasi*-alchymy, and in *direct* oceanic precipitations unknown in nature. The *mineralogist* must assume certain obscure and insufficiently tested bodies to consist of calcite: he must be inappreciative of the various allomorphs of serpentine, and of pseudomorphic phenomena; and consider every imbedded crystalline body—"tuberculated," or "segmented"—"cylindrically shaped," or with angles rounded off—to be the remains of an organism. The *palæontologist*, besides slighting all he knows of the circumstances of petrifaction, must accept as a "fossil" a production never found in rocks that ought to contain it. Even the *zoologist* must believe to be a "*nummuline* foraminifer" what is structurally an *Impossibilitas Naturæ*, in having a "canal system" and "skeleton" that often "*run wild*" without either "chambers" or a "cell wall"; and in being seldom otherwise than *inconceivably* the result of pseudopodial tubulation.

* Paper on Glacial Striation read before the Geological Section of the British Association, 1865.

† Geological Magazine for June, 1870, p. 276.

4. *On Fossil Remains of Mammals found in China*, by Prof. OWEN, (Q. J. Geol. Soc., for Aug., 1870).—Prof. Owen describes in this paper the following species: 1. *Stegodon Sinensis* Ow., from a tooth obtained "from marly beds in the vicinity of Shanghai, probably Tertiary," (belonging to the group of Elephants called "transitional forms" by Owen in his Odontography, which Falconer defined and named Stegodon); 2. *S. orientalis* Ow., said to be "from a cave near the city of Chung-tung-foo, Province of Szochuen," and having a more recent aspect than the tooth of *S. Sinensis*; 3. *Hyæna Sinensis* Ow., from specimens supposed to come from a Chinese cavern; 4. *Rhinoceros Sinensis* Ow., probably from the same cavern; 5. *Tapirus Sinensis* Ow., id.; 6. *Chalicotherium Sinensis* Ow., id. The specimens, excepting that of the *S. Sinensis*, were obtained by Mr. Swinhoe from apothecaries in China; and Prof. Owen observed that out of the great quantity of fossils that had thus come into his hands, he had selected for description those agreeing in chemical and other characters, and which "might justly be inferred to be of the same age, and to be derived from the cave mentioned by Mr. Swinhoe."

5. *Diamonds of South Africa.*—According to Dr. JOHN SHAW. as we learn from *Nature*, Nov. 3, the Vaal region, now famous for its diamonds, contains syenite and other metamorphic rocks, quartzite, micaceous sandstones, and clay schists, with trap or basalt and trap conglomerate. The diamonds are found in alluvial gravel, and this gravel contains pebbles of quartzite, granite, clay-slate, garnet, tourmaline, spinel, agate, iron pyrites, the whole waterworn and polished, and imbedded, at Klipdrift, in a brownish fatty clay. Diamonds have been found two hours distance from Potchefstroom, and all down the Vaal to its junction with the Orange river, and thence to ten hours distance below Hope Town, a range of at least 500 miles. Dr. Shaw is of the opinion that the diamonds have come "from some rock which may now have vanished, but which existed formerly through the whole region." The old diggers prefer for their washings the summits of the Kopjes, where the gravel is of wide extent and is far above the influences of the river. These summits are all basalt. In the hollows no gravel is open to view, it being covered with sand, "the accumulation of present denudation;" and thus far the diggers have not undertaken the labor of carrying off the surface sand for work below. "The present diamond digging of South Africa is therefore only trifling in comparison to what it should and will ultimately be."

6. *Publications of the Geological Survey of India, under the direction of* THOMAS OLDHAM, LL.D.—Published by order of His Excellency, the Governor General of India, in Council.—These publications now include six volumes of Memoirs in large octavo; a thick quarto volume with many plates, under the general title Palæontologica Indica, on the Cretaceous Gasteropoda, by F. Stolicka; another, on the Cretaceous Cephalopoda, by H. F. Blanford and F. Stolicka; and parts of two others, on the Fossil Flora of the Rajmahal Hills, Bengal, by T. Oldham and John Morris, and on the Vertebrate Fossils from the Panchet Rocks, Bengal, by

T. H. Huxley; also brief Annual Reports of the Survey; also Catalogues of the Meteorites, and of some departments of Organic remains in the Museum of the Geological Survey of India. The Survey also issues "Records" of the survey, consisting of short papers, in large 8vo, at intervals of about three months. The first number of the second volume (1869) contains 24 closely printed pages. Subscriptions for the year 4 shillings sterling. The address for the publications of the survey is Superintendent of Geological Survey of India, Hastings street, Calcutta. The present year 1870, is the 15th of the survey.

7. *Geological Survey of California;* Ornithology, vol. I. Edited by S. F. Baird from the manuscript and notes of J. G. Cooper. 592 pages, royal 8vo, with 662 cuts. 1870. (Soon to be issued, and sold by Little, Brown & Co., Boston; uncolored, $10).—This admirable report is far in advance of any similar work on Ornithology hitherto published in this country, if not in Europe, and does great honor both to the state of California and the officers of the Geological Survey. The first volume contains descriptions of all the land birds hitherto found in the region north of Mexico and west of the Rocky Mountains. Each genus is illustrated by a reduced full length cut of one of the species, and by natural size cuts of the wing, tail, bill, and foot, which will render it very easy for any one to recognize each genus, while nearly all the species are illustrated by full size cuts of the heads, often of both male and female. The cuts are nearly all original and have been drawn and engraved with great care and skill. The cuts are, in our estimation, superior both in accuracy and beauty to any hitherto published in any work on ornithology, and are far more satisfactory than the highly colored, but often coarse and inaccurate, lithographs, so often employed to illustrate ornithological works. Much information in regard to the habits and geographical distribution of the species has been contributed by Mr. Cooper. One feature, rather novel in general works on ornithology, though in accordance with the views of many modern ornithologists, is a decided change in the systematic arrangement of the orders and families. The singing birds are placed in the first rank, where they doubtless belong, while the rapacious birds take a much inferior position, next to the Rasores, instead of being placed at the head of the whole class, as is usually the case. An edition with colored illustrations is in course of preparation. v.

8. *Geological Chart of Sweden.*—Sheets 31 to 35 inclusive of the Geological chart of Sweden have recently been issued. Nothing can exceed the beauty and perfection of coloring, or the apparent exactness and fullness of scientific details. As there is no department of art in which this country is more behind hand than that of map coloring, and none of Science so disgraceful to us, as that of Geological Charts, we could wish that a copy of the chart might be in the hands, not only of all Geologists, but also, of all members of Congress and State legislatures. The only commendable geological map ever published on this continent is the Canadian, and that was engraved and printed in Paris.

9. *Delesse's Lithologie des Mers.*—Besides the European chart, noticed in a recent volume of this Journal, Delesse has issued one of North America. It is a beautifully colored chart, and illustrates the lithology of the bottoms of the seas as far as known to him, together with the hydrographic basins. His chartist has not been as careful as is desirable for such work. He has joined the head of the Hudson river above Albany to the Chesapeake; while the Hudson River itself he has dwindled to a little stream rising in western Connecticut. The errors probably crept in through carelessness in tracing off from another chart. But there is other want of exactness, showing that the chart needs careful revision. No text for it has yet been issued.

III. Astronomy.

1. *On the Mass of Asteroids between Mars and Jupiter;* by Prof. Daniel Kirkwood, Bloomington, Indiana.—According to Leverrier, the total mass of the ring of minor planets does not exceed one-fourth of the earth's mass, or $\frac{1}{1200}$th of that of Jupiter. So great a disproportion between two adjacent planets is without a parallel. Is the fact susceptible of a probable explanation?

Were the sun transformed into a gaseous spheroid with an equatorial radius equal to the diameter of the earth's orbit, a large number of the known asteriods would, in perihelio, plunge into the solar mass and be reunited with it. Now this, in all probability, is precisely what occurred soon after the abandonment of the asteroid zone, while the solar nebula was in process of condensation. The powerful mass of Jupiter would produce great eccentricity in parts, at least, of the primitive ring. Large portions of its matter, or a considerable number of minor planets in a state of vapor, may thus have been precipitated upon the sun before the latter had contracted within their perihelion distance. The small mass of Mars may perhaps be accounted for on the same hypothesis.—*Proc. Phil. Soc. Philad*

2. *Astronomical and Meteorological Observations made at the United States Naval Observatory during the year* 1867; published by authority of the Hon. Secretary of the Navy, Commodore B. F. Sands, Superintendent. Washington, Gov. Printing Office, 1870.—The Astronomical observations of the year 1867 at the Naval Observatory, together with the computed positions of the objects observed, fill over 400 closely printed quarto pages in this volume, and the meteorological observations about 50 pages.

To these results of routine work are added four important reports as appendices.

App. 1. Difference of Longitude between Washington and Ha vana; by Prof. W. Harkness, U. S. N. App. 2. Observations of the total eclipse of the Sun of Aug. 7th, 1869. App. 3. Positions of the Fundamental Stars, deduced from Washington observations made between the years 1862 and 1867 inclusive; by Prof. S. Newcomb, U. S. N. App. 4. Catalogue of 151 stars in Præsepe, by Prof. A. Hall, U. S. N.

Prof. Harkness gives for the position of Morro Light, lat. 23°

9′ 21″·0 North, lon. 0^h 21^m 13^s·43 West, or 82° 21′ 20″·4 W. of Greenwich. The latest edition of the Admiralty list of lights in the West Indies, gives 82° 22′ 12″ for the longitude, nearly a mile in error.

The second appendix has been noticed in this Journal, (II, vol. xlix, p. 134).

We cannot too highly commend in the management of the Naval Observatory, this practice of publishing memoirs prepared by the Assistant Professors *under the name of the authors.* The personal interest in the careful elaboration of the subjects which is thus secured is of great value to science. This practice has been most injuriously and unjustly departed from in more than one observatory in times past.

IV. Miscellaneous Scientific Intelligence.

1. *Solar Eclipse of Dec.* 22, 1870.—Congress having appropriated $29,000 to enable the Superintendent of the Coast Survey to observe the total eclipse of the sun, which will be visible on the shores of the Mediterranean on the 22nd of Dec. next, Professor Peirce has organized two parties, one which, under his own lead, will occupy several points in Sicily, while the other, under the direction of Prof. Joseph Winlock, director of the Harvard observatory, will occupy points near the line of totality in Spain.

Prof. Peirce is accompanied by Messrs. C. A. Schott, J. H. Lane and Chas. S. Peirce of the Coast Survey, Major H. L. Abbott and Capt. O. H. Ernst of the U. S. Engineers, Prof. J. C. Watson of the University of Michigan, Dr. C. H. F. Peters of Hamilton College, Prof. W. Eimbeck of St. Louis, and Messrs. H. G. Fitz and D. C. Chapman of New York, the two latter having been provided with outfit and instructions for photographic observations by Mr. Lewis M. Rutherfurd of New York.

Prof. Winlock's party consists of G. W. Dean, assistant in the Coast Survey, Professors C. A. Young of Dartmouth College, E. P. Pickering of the Mass. Inst. of Technology, S. P. Langley of the Western University of Penn. at Alleghany, Mr. Alvan G. Clark of Cambridge, and Mr. O. H Willard, photographer of Philadelphia, together with four aids.

On the part of the U. S. Naval Observatory four observers have also been sent out, viz: Professors S. Newcomb, A. Hall, W. Harkness and J. R. Eastman. J. E. H.

Four parties have left England under the auspices of the British Government, one to Cadiz in charge of Rev. S. J. Perry; one to Gibraltar under Captain Noble; a third to Oran (Algiers) under Mr. Huggins; and a fourth to Sicily, under Mr. Lockyer. "*Nature*" of Nov. 24 states that "Professors Young, Pickering, Newcomb, Peters, Watson, Harkness, and others are at present in London, and are daily affording most valuable information to the Organizing Committee and the various observers." While the British Government were hesitating with regard to sending expeditions for observations Prof. Peirce extended an invitation to the English astronomers to join his parties.

2. *A Topographical Survey of the Hawaian Islands* has been ordered by the Legislature of the Islands, and an appropriation of $5000 made for procuring instruments and meeting the expenses of the first year. Prof. W. D. Alexander of Oahu has been appointed Surveyor General, and is making arrangements for commencing the work. He proposes to measure a base line on the sandy isthmus between East and West Maui, which is six or seven miles wide, and to carry forward the survey as nearly as possible after the methods of the U. S. Coast Survey. Geological and Botanical collections and observations will be made in connection with the survey.

3. *A Central Observatory.*—Prof. H. A. Newton has suggested that an excellent position for the great central observatory of the continent is a few miles east of St. Louis, on the 90th meridian from Greenwich. American time reckoned from such an observatory would differ from Greenwich time just six hours, and American longitudes from standard longitude from Greenwich by ninety degrees. It is believed that there are good locations, as regards atmospheric and other conditions, on that meridian not far from St. Louis.

4. *Telescopes.*—Messrs. Alvan Clark & Sons have received recently two orders for telescopes of about 25-inch object glass; one from Mr. McCormick, and the other from the National Government.

5. *Auroral Belt of Oct. 24–25th.*—An account of the remarkable crimson belt, in the great auroral display of Oct. 24–25th, by Prof. A. C. Twining, is deferred to the next number for want of space.

OBITUARY.

Dr. Matthiessen.—The death of Dr. Augustus Matthiessen will be learned with sorrow by those of our countrymen who knew him in Heidelberg, in the years of his student-life; or who received from him afterward in London, the attentions which his kindness of heart made him ready to bestow.

Dr. Matthiessen's mind was peculiarly fitted for scientific research. The acuteness which leads to the perception of truth, detects as well the sources of error and suggests the means by which they may be avoided. With the most persevering industry he had a fondness for experimental enquiry; and as his mind moved easily, he was capable of long continued exertion.

The work which first brought him into notice, was a memoir upon the "Preparation and Properties of the Metals of alkaline Earths," made while yet a student in the laboratory at Heidelberg. Prof. Bunsen had himself obtained the metals lithium and magnesium; to them Dr. Matthiessen now added strontium and calcium. After his return to London he began an investigation of the conducting powers of the metals and their alloys for electricity, which occupied him several years, and was afterwards extended to their conducting powers for heat. He subsequently examined the chemical properties of the alloys of zinc with bismuth and with lead; and an alloy of tin and gold which he had obtained in crystalline form. Upon these subjects he delivered in 1867, a lecture before the Chemical Society of London, which was repeated

before the Royal Society in the following year. He also investigated the chemical constitution of the opium bases and their products of decomposition, and made elaborate determinations of the expansion by heat of mercury and water. For his various researches the Royal Society, of which he was early chosen a fellow, awarded him in 1869, its gold medal.

In the fifteen years which comprise his scientific life Dr. Matthiessen published at least twenty-eight memoirs. That he was able to accomplish so much was owing in great part to a judicious economy of time, not the penuriousness which refuses hours of relaxation to days of labor, but to a right choice, of subjects which would repay investigation, and of the most direct means by which the investigation could be carried out. He had also the faculty of availing himself of the powers of others by associating them with his work.

With remarkable acuteness of mind, or shrewdness, Dr. Matthiessen had a sincere and affectionate disposition. Resolved from the first to make his own way, he was always ready to help on others. He was a kind, a firm, a generous friend; generous of his thoughts, his means, and his influence. At the time of his death, no man of his age in England stood in a higher position in science; nor was there one from whose future, judging from his past, more might with reason have been expected. D.

Mr. Edward Hartley, Mining Engineer of the Geological Survey of Canada, died in Pictou, Nova Scotia, on the 10th of November last, aged twenty-three years. Mr. Hartley was the eldest son of Mr. William M. B. Hartley of New York, and grandson of Mr. Philos Blake of New Haven. He early showed a special aptitude for the study of the natural and physical sciences, and for mechanics. At the age of fifteen he became a student in the Sheffield Scientific School of Yale College. On leaving the school, though still very young, he was at once charged with the examination and surveying of mineral lands in Maryland and Pennsylvania, and subsequently with the erection of machinery for working gold in North Carolina. His abilities attracted the attention of the officers of the Geological Survey of Canada under Sir W. E. Logan, and in July, 1868, he joined the Survey as a geological assistant. The following year he was appointed Mining Engineer to the Geological Survey. His duties from this time confined him to the coal-fields of Nova Scotia, where in 1868 he worked conjointly with Sir W. E. Logan, and in 1869, alone, completing a careful and detailed Survey of the Pictou coal-basin, of which an elaborate report by Sir William and another by himself was printed and privately distributed before his death. It will be published with a map in the forthcoming volume of the Geological Survey.

During 1870, Mr. Hartley was engaged with an assistant in the Survey of the Cumberland coal-basin in Nova Scotia, and of the Cape Breton collieries, and had nearly completed his labors for the season when he died after an illness of but six days duration, brought on, probably, by labors beyond his physical strength.

T. S. H.

EDWIN W. ROOT, Prof. of Chemistry in Hamilton College, died Nov. 15, in the 30th year of his age. On returning to this country in 1865, after his studies in Germany, he was appointed Assistant Professor of Analytical Chemistry in the School of Mines of Columbia College. Afterward, in 1868, he became Professor of Agricultural Chemistry, and the next year of General Chemistry, in Hamilton College. Prof. Root was highly esteemed for his scientific and literary attainments, integrity of character, and tested ability.

V. MISCELLANEOUS BIBLIOGRAPHY.

1. *Storer's (Frank H.) Cyclopædia of Quantitative Analysis;* Part I, 112 pp. 8vo, with an appendix of 8 pages. Boston and Cambridge, 1870. Sever, Francis & Co.—Prof. Storer states in his preface that his object in preparing this book has been not only to provide the student and working chemist with a comprehensive dictionary of quantitative processes, but to show also the possibility of presenting this branch of chemical art in a more serviceable and manageable form than has been customary hitherto: and he adds, 'the experiment is certainly worth trying whether a definite system of classifying substances in alphabetical order, and of referring each and every process to the fundamental fact or *principle* upon which it depends, will not greatly facilitate both the study and the practice of Analysis.' An examination of this first part of Prof. Storer's "Cyclopædia" reveals at once the hand of a master as distinguished from the work of the compiler. The alphabetical order by no means hampers the author's philosophical treatment of subjects. This is particularly well illustrated in the articles "CARBON" and "CARBONIC ACID," where the originality of the method of treatment is surpassed only by the thoroughness of the detail given of all methods, of any value, known to chemists, for the determination of carbon whether as such—as in the analysis of cast-iron and steel—or by its oxidation to carbonic acid and estimation in any mixture or compound. The author's entire familiarity with the whole field of chemical literature has enabled him to render a justice to American researches in analytical chemistry which has never been awarded by any foreign author.

2. *The Story of the Rocks. A Fourteen Weeks' Course in Popular Geology;* by J. DORMAN STEELE, A.M., Ph.D., Principal of Elmira Free Academy, author of a Fourteen Weeks' Course in Chemistry, etc. 280 pp. 12mo. 1871. New York and Chicago. (A. S. Barnes & Co.).—This small geological text-book will be read with interest by those that are not critical, and are satisfied with so brief a presentation of the science. While in many respects well arranged for instruction, and clear in its descriptions, its statements are often wanting in exactness, and its explanations unsatisfactory. The following are a few examples. "Rocks are composed in general of only three common minerals, *Quartz*, *Clay* and *Lime*." But clay is not a mineral; lime should be *limestone*; and granite and all the crystalline rocks have no place under such a definition.

Again, "Oolite is a limestone containing [should be, consisting of, although the bad drawing given sustains the author] numerous, small rounded [add concretionary] grains, resembling the roe of a fish." He says that, "The coral animal thrives best in pure water, about a hundred feet in depth," in place of the fact that reef-forming corals thrive well at low-tide level and at all depths below down to one hundred feet, while other coral animals occur at depths exceeding a thousand feet. The author attributes the "continually vibrating to and fro" of the continent, which finally culminated in the folding of the rocks producing the Appalachians, to "the tremendous pressure of the two oceans during the Carboniferous age." Bad hydrostatics, this. A drawing of an Ichthyosaurus represents him spouting like a Right Whale!

3. *A Text Book of Elementary Chemistry, Theoretical and Inorganic;* by GEORGE F. BARKER, M.D., Professor of Physiological Chemistry in Yale College, New Haven, Conn. vi and 342 pp. 12mo. New Haven, 1870. (C. C. Chatfield & Co.)—Prof. Barker has produced a compact Elementary Text Book, the dimensions of which adapt it to the use of schools, and which is the first in our language wherein "Modern Chemistry" is presented systematically. The style of the work is concise and animated, the illustrations are fresh, the typography is good, and it cannot fail of a hearty welcome among our teachers and learners.

4. *The American Naturalist, a popular Illustrated Magazine of Natural History.* (Salem, Mass. $4.00 a year).—This monthly Magazine of Naturalist History has won for itself a high place among the Journals of the world. It knows how to popularize science without degrading it. Many of its articles are by some of the best zoologists of the country, and contain the results of original observation. The illustrations are always excellent.

A Monograph of N. A. Astacidæ, by Dr. Hermann A. Hagen; No. III, of the Illustrated Catalogue of the Museum of Comparative Zoology of Harvard College. (Published by order of the Legislature of Massachusetts). 112 pp. roy. 8vo, with 11 plates. Cambridge, 1870. To be noticed in our next No

Students' Manual of Comparative Anatomy, and Guide to Dissection, by G. Herbert Morrell, B.A., B.C.L. Longmans, Green & Co. Aves, ready. Mammalia promised for November.

Elementary Treatise on Natural Philosophy, by Sir W. Thomson and Prof. Tait. In the press. Macmillan & Co.

The Petroleum Monthly, devoted to the interests of the Oil Business. S. P. Irvin and J. H. Bowman. Editors and Publishers, Oil City, Pa. The first number of this Journal appeared on the first of November. Price three dollars a year.

Catalogue of the Principal Minerals of Colorado, with notes by J. Alden Smith. 16 pp. 8vo. Central City, 1870.

A Monograph of the Alcedinidæ: or, Kingfishers, by R. B. Sharpe, F.L.S., &c., Librarian to the Zoological Society of London, to be complete in fifteen parts. Price to subscribers, 10s. 6d. each. Annual subscription, £2 2s. Only thirty copies now remain of this work. The price will be raised when the work is completed, which will soon be. Intending subscribers are respectfully requested to send their names to the author, at "The Zoological Society of London," 11 Hanover Square, London, W. Also, uniform with the above, A Monograph of the Capitonidæ: or, Scansorial Barbets by C. H. T. Marshall, F.Z.S., and G. F. L. Marshall, F.Z.S. To be completed in nine parts. Price 10*s.* 6*d.* each.